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CONTENTS

The Role of Hedonic Processes in the Organization of Behavior ..... PAUL THOMAS YOUNG 249
"Dynamic Systems" and Theory Construction ..... WILLIAM KESSEN, GREGORY A. KIMBLE 263
A Note on "'Dynamic Systems' and Theory Construction" ..... DAVID KRECH 268
"What Is Learned?"—A Theoretical Blind Alley ..... HOWARD H. KENDLER 269
A Unifying Theoretical Approach to Motor Learning ..... HILTON N. WASSERMAN 278
Objectivity in Systematic and "Idiodynamic" Psychology ..... WILLIAM SEEMAN, EUGENE GALANTER 285
The Formation of Negative Habits: A Neurophysiological View ..... JOSEPH WOLPE 290
Three Estimates of Differential Excitatory Tendency ..... FRANK A. LOGAN 300
Perceptual Defense ..... DUNCAN HOWIE 308
A Note on One-Tailed and Two-Tailed Tests ..... W. E. HICK 316
Some Remarks on the Ames Oscillatory Effect ..... NICHOLAS PASTORE 319
Toward a General Theory of Perception ..... HEINZ WERNER, SEYMOUR WAPNER 324
The Investigation of Repression As an Instance of Experimental Idiodynamics ..... SAUL ROSENZWEIG 339

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THE PSYCHOLOGICAL REVIEW

THE ROLE OF HEDONIC PROCESSES IN THE ORGANIZATION OF BEHAVIOR [1]

BY PAUL THOMAS YOUNG

University of Illinois

Current theoretical and experimental work stemming from Hull (5), Miller (8), and others, is based on the assumption that reinforcement comes from need-reduction or from an associated drive-reduction. Underlying this work is the hypothesis that motivation can be adequately described in terms of stimulus and response. Miller writes: "Thus the basic mechanism of motivation (strong stimulation) is the same for primary and learned drives, and the basic mechanism of reinforcement (a reduction in strong stimulation) the same for primary and learned rewards" (8, p. 440).

We believe that the stimulus-response formula for motivation and learning is not fully adequate because it neglects central hedonic processes. Central hedonic processes are associated with certain stimuli and also with certain responses but they can not be identified either with stimuli or with responses. Hedonic processes must be considered in their own right.

The purpose of the present paper is to examine the role of hedonic processes in the organization (or disorganization) of behavior. The *affective-determination* theory of food-seeking behavior was presented in another paper (19, pp. 108–110) and contrasted with the prevailing *need-stimulus* theory of drive. The present article will re-state the theory, elaborate it in some respects, and point to the feasibility of testing hedonic theory experimentally.

[1] The writer would like to acknowledge the helpful criticisms and suggestions of his colleagues who have read this paper: Drs. G. R. Grice, A. L. Irion, O. H. Mowrer, and L. I. O'Kelly.

Affectivity and Need in the Organization of Drives

The dietary requirements of man and animal have been extensively studied by nutritionists. In general, dietary needs can be described in detail by referring to patterns of deficiency symptoms that result when an animal is deprived of a specific nutritive substance. For example, when there is a deficiency of vitamin A in the diet there is a disturbance of bone growth, sterility, cessation of normal growth, and a failure in the metabolism of visual purple which results in night blindness. Some of the deficiency symptoms (not all of them) are of interest and importance to the psychologist. In addition to deficiency symptoms there are symptoms of excessive and "unwise" eating.

Metabolic *need* is an objective concept, important in the field of nutrition. To describe needs there must be some criterion such as survival, maintaining homeostasis, growth, reproduction, response to stimulation, etc. *Need* is always relative to a criterion; there is need *of* something *for* something. To

illustrate, if maintaining homeostasis is the criterion, then the organism *needs* water, oxygen, protein, sodium, vitamin A, and other dietary elements. If reproduction is the criterion, then the laboratory animal *needs* vitamin E. Metabolic *needs* are of interest to the psychologist in so far as they relate to behavior; but some nutritional needs affect growth without appreciable relation to behavior.

The primary drives are commonly referred to their corresponding primary needs. Thus Hull (5) in his much discussed *Principles of Behavior* described a number of primary needs and their associated drives, e.g., the need for food (hunger), the need for water (thirst), the need to avoid tissue injury (pain), etc. *Need*, for Hull, refers to a tissue condition resulting from deprivation or damaging stimulation. It is the drive stimulus (S_D), or afferent discharge characteristic of a need, that motivates behavior.

Reduction of the drive-stimulus constitutes reinforcement. Primary reinforcement occurs when there is diminution of a primary drive. Drive-reduction reinforces those stimulus-response events that accompany it or (in secondary reinforcement) that are associated with it. For Hull, reinforcement through drive-reduction is a basic principle of learning.

Hullian analysis permits a distinction between *need* for food (an objective nutritional concept) and hunger *drive* (primary motivation). Although Hull appears not always to distinguish clearly between need and drive, the distinction can be drawn without impairing in any way his argument and analysis. It is, however, the assumption that drive-reduction "reinforces" behavior that causes trouble.

MacKinnon (6), in a clear discussion of motivation, begins by considering the needs of man. He then defines need as *"A tension within an organism which tends to organize the field of the organism with respect to certain incentives or goals and to incite activity directly toward their attainment"* (p. 126). This is straightforward; but if *need* is a tension, the concept is clearly equivalent to *motive* or *physiological drive*. It is necessary, we are convinced, to distinguish between need and drive.

In a well-known investigation with the Columbia University obstruction apparatus it was observed that continuing deprivation of food brought an increased strength of behavioral food-oriented drive but after 2 to 4 days the strength of drive decreased. If we consider starvation from the point of view of bodily need, it is obvious that need increases steadily until death by starvation. The behavioral drive of a dying animal, however, is low; he is too depleted to cross the grill and take punishment. In a prolonged human fast it is known that there is first an increasing distress from stomach cramps; later these cramps disappear and the subject experiences weakness and lassitude. The manifest strength of behavioral drive is, in fact, more closely related to the degree of experienced distress than it is to the degree of bodily need.

Two recent experiments from the Yale laboratories show the weakness of the drive-reduction hypothesis. First, Sheffield and Roby (15) in a study with rats have confirmed the fact that saccharine, a non-nourishing but sweet-tasting substance, is effective as a reward in instrumental learning. The reward value of a 0.13 per cent saccharine solution, incidentally, was found to vary with the state of hunger. Interpreting their findings, Sheffield and Roby suggest that elicitation of the consummatory response is more critical as a *primary* reinforcing factor in instrumental learning than the drive-reduction that is subsequently achieved. Second, Sheffield, Wulff, and

Backer (16) reported that copulation without sexual drive-reduction has a reward value. Sexually mature male rats copulate several times with a receptive female before ejaculation and drive-reduction occurs. This fact makes it possible to permit copulatory activity that does not lead to drive-reduction. Sheffield, Wulff, and Backer have shown that copulation without ejaculation and without a past history of ejaculation, functions as an effective reward for selective learning in male rats. Further, the strength of the acquired instrumental response was closely related to the strength of the copulatory tendency. They argue that the elicitation of a dependable and prepotent consummatory response (copulation) is the critical factor in reinforcement rather than drive-reduction or beneception.

These two experiments raise a question of interpretation. What is it that sweet-tasting saccharine and copulation have in common? The answer does not require the imaginative capacity of a genius: both activities are affectively positive, in human terms, enjoyable.

In this connection reference is here made to an unusual finding of Scott, Verney, and Morissey (14). In carefully controlled experiments they found that magnesium-deficient rats *avoided* diets containing magnesium despite the fact that this mineral is needed for ultimate survival. Commenting upon the finding they write: "The most plausible explanation of these results is that the avoidance of magnesium is learned. It is well-known that a feeling of well-being is not always associated with the best possible physical status nor the ultimate welfare of an individual. Examples that may be cited are those of alcoholism, narcotism, and the euphoria that may occur in high altitude anoxia. Magnesium-deficient animals are nervous and highly excitable and likely to go into convulsions on slight stimulation, while magnesium is a nervous system depressant. It is possible that the highly excitable stage of magnesium deficiency is pleasurable to the rat, and that he learns to avoid a diet that represses this pleasurable state . . ." (14, p. 199).

Typically a *need*, a deficiency state, leads to random movements. Moderate magnesium deficiency appears to be an exception to the rule.

In any event, *need* must be distinguished from *drive* since under some conditions the two can be dissociated. Further, we are assuming that both need and drive must be distinguished from affective *distress*. Although distress is typically associated with need, there are exceptions to the rule. We assume that distress is a motivating condition that leads to action to relieve it. It is the *relief* from distress, we assume, that results in the organization of neurobehavioral patterns.

The relation between relief from distress and the development of a specific appetite is illustrated in the pioneer experiments of Harris *et al.* (3). These investigators found that if rats are deprived of vitamin B_1, they fail to select a diet containing a small but sufficient amount of this vitamin complex. If, however, depleted rats are fed for a few days upon an adequate diet and permitted to associate its beneficial and curative effects with some flavor, they continue to select the adequate, flavored diet when it is presented along with others. They continue to choose the flavored food even after the vitamin has been withdrawn so that the flavored diet is inadequate. Harris *et al.* argue that *relief* from the symptoms of depletion must be experienced and associated with some characteristic of the foodstuff for a food habit to develop. The association is not an immediate one but it requires time to establish.

This result has been confirmed by Scott and Quint (12), with better con-

trol of conditions. They trained rats to swallow daily a vitamin-containing pill, the contents of which could be accurately controlled. By removing from the pills a specific vitamin (thiamine or riboflavin or pyridoxine or pantothenate) a vitamin deficiency could be produced. Each cage contained two foodcups—one containing the standard diet, and the other the same diet plus the vitamin being tested. During a three-week control period the diet was supplemented by vitamins in the pills. During the experimental period the pill technique was continued but a vitamin was removed and added to one of the test-foods. Under the experimental conditions a vitamin deficiency would steadily develop unless the rats could learn to select the vitamin-containing diet.

Scott and Quint found that when rats are depleted of thiamine or riboflavin or pyridoxine they develop an appetite for the missing vitamin. They believe that this is a *learned appetite* that develops only under conditions of need. Rats maintained upon a vitamin-adequate diet do not develop a preference for the vitamin-containing food. Pantothenate was different from the other vitamins in that a specific appetite developed only when the adequate diet was labeled with an odor; even then some of the deficient animals failed to select the vitamin-containing food.

In a further study of vitamin appetites Scott and Verney (13) suggest two reasons why the deficient rats continue to eat the vitamin-containing diet: (1) A habit is set up involving the association of a feeling of well-being with some characteristic of the diet, and (2) there is a stimulus to eat the diet, presumably derived from the feeling of well-being, that persists over a considerable period of time.

The above experiments show clearly that although need and drive are related, food-seeking and sexual activities are not organized solely to reduce needs. The investigators themselves sometimes refer to *affective processes* as the probable basis for organization of food-seeking and sexual patterns.

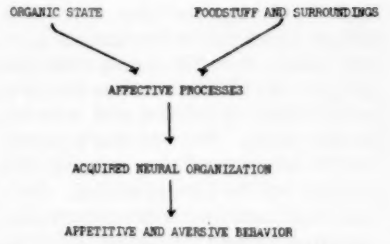

FIG. 1. Central importance of affective processes in the organization of behavior.

In the area of food acceptance affective processes enter the picture in two ways: first, there is an immediate affective arousal when the animal makes sensory contact with the food (palatability effect); second, there is a delayed relief from organic distress that is based upon deficiency conditions (need). Usually when a food is needed that food is raised in palatability, but this relationship is not always found. What a rat needs is one thing; what he accepts is something else. In the last analysis, we believe, it is the *affective arousal* that organizes food-oriented appetites and aversions.

The central importance of affective processes in the organization of behavior is diagrammed in Fig. 1.

Fig. 1 shows, at the top, that affective processes are dependent upon two main groups of conditions. First, there is the organic state at the time appetitive or aversive behavior appears. This organic state depends not only upon deficiency conditions of need; it depends also upon excessive eating, "unwise" eating (e.g., green apples), diseases, injuries, constitutional conditions, etc. Second, affective processes depend upon the characteristics of the foodstuff and its surroundings. The term *palatability* recog-

nizes that when an animal makes contact with a foodstuff there is an immediate affective arousal that depends upon the kind of food, the temperature, texture, concentration of a solution, etc. The concept of affective process, of course, is much broader than that of palatability since enjoyment is also associated with sexual activity, free play, success in various undertakings, and since *relief* from distress is also an affective process.

Fig. 1 shows that the concept of need-reduction or drive-reduction is not sufficiently broad as a basis for motivational theory. A broader basis is found in the theory that neurobehavioral patterns are organized to relieve distress and to enhance (heighten, prolong) enjoyment.

Identification of Affective Processes

The terms *pleasant* and *unpleasant* are clearly derived from human conscious experience. If we are to discuss the affective processes of animals from a strictly objective point of view, it is necessary to ask how they can be recognized and operationally defined.

It would be a mistake to identify affective processes with adient and abient behavior for such behavior is *learned*. When a newborn infant is deprived of food or water or when he is pricked by a pin he simply cries or screams or kicks at random. Sooner or later he learns instrumental acts that relieve distress. When an animal tastes a sweet substance he continues ingesting it; he organizes a central neural determination that enhances (heightens, prolongs) enjoyment. The instrumental acts that develop are organized, we believe, to relieve distress or to enhance enjoyment.

From an objective point of view it can be seen that some patterns of behavior (drinking sweet-tasting fluid, copulating, playful behavior, etc.) are continued persistently by the animal. He does nothing to avoid them and he acts to continue or preserve them. Other activities are directed against the stimulations evoking them. For example, the rat works to avoid a bright light or to avoid painful stimulation.

One practical difficulty in the objective study of affective processes is a possible confusion between affective process and appetitive or aversive behavior that has been developed on the basis of affectivity. The difficulty can be illustrated by reference to a study of food preferences. In one experiment rats developed a preference of sugar to casein. The protein was then removed from the diet to heighten the need for casein. On the standard apparatus well-habituated rats continued to select sugar in preference to casein despite a manifest need for the non-preferred food. On another apparatus the reverse preference *developed;* the *same* animals showed a preference for casein (agreeing with their needs). There were two simultaneous and opposed preferences! After studying the matter we concluded that the *developing* preference (the one consistent with bodily needs) revealed true acceptability relations and that the preference for sugar was, in this instance, an established preferential habit opposed both to needs and to changed palatability relations.

It is necessary, therefore, to approach the objective study of affective processes in a time-wise manner for it is the *organizing* of adient and abient behavior that indicates affectivity rather than the existence of adient and abient behavior. The practical test is this: Does the naive animal *learn* patterns of behavior that continue a reaction or that avoid it? If he does neither, the reaction may be assumed to be affectively indifferent.

In the experimental study of affective processes it should be remembered that enjoyment and distress cannot be identified with any specific reactions or stimuli. Tasting a sweet substance, as we

said, is pleasing, but the pleasantness cannot be identified either with the substance itself or with the tasting response.

From the objective point of view affective processes are hypothetical central (neural) events. They cannot be perfectly correlated with any pattern of response or with any stimulus pattern. They are, however, assumed on the basis of observed behavioral development.

Ordinarily the patterns of crying, screaming, struggling, are assumed to indicate unpleasant disturbance in the infant. Smiling, laughing, cooing, are taken as marks of pleasant experience. The objective test, however, lies in the fact that an infant *learns* to avoid some stimulations and activities and to continue others.

Inadequacy of Physical Concepts

If we approach the analysis of behavior with the objectivity of a physicist, we observe that foodstuffs offered to animals as rewards differ in ***kind*** and ***amount***. The kind of food can be described in physical and chemical terms. The amount can be measured in grams or it can be expressed as a number of uniform food pellets. This physical approach to problems of food acceptance misses a fine point: Kind and amount of food are physical concepts; palatability is psychological.

The relation between kind of test-food and acceptability appears clearly in experiments upon food preference. First, if a rat is permitted to eat the preferred food prior to a test of preference, a preference can be reversed. The level of acceptability gradually falls with approach to satiation while the ***kind*** of food is physically constant. Second, if a rat is deprived of a non-preferred food (one that is needed to maintain homeostasis) the level of acceptability rises and the original preference reverses in agreement with needs. Again, acceptability changes while the ***kind*** of food is constant. Third, it commonly happens that a rat fails to show a preferential discrimination between two test-foods but eventually a preference develops. In this situation the test-foods are different in ***kind*** but (for a considerable period of time) they do not differ in level of acceptability. ***Kind*** of food is one thing; *acceptability* is something else.

There is no doubt that the *level* of performance varies with the ***amount*** of food presented as a reward. Crespi (2), Reynolds (11), and others, have demonstrated the point. A question should be raised, however, concerning the interpretation of these results.

As a rat continues ingesting a test-food there is approach to satiation during which the level of acceptability falls. The characteristics of approach to satiation vary from food to food and somewhat from rat to rat. For this reason preferences measured by the weight or volume of food consumed are of uncertain significance. In one experiment we found that in a brief test rats consumed more sugar than wheat but if the test was extended to an hour, they consumed more wheat. The explanation is that the animals initially preferred the sugar but were quickly satiated upon it and then turned to the wheat. On the basis of the ***amount*** of food consumed the "preference" varied with the duration of the test.

Quite apart from the approach to satiation with its reduction in acceptance level it is clear that the duration of the feeding process (and presumably of enjoyment) is directly related to the quantity of food consumed. Since enjoyment of food is dependent upon contact with it and actual eating, it is obvious that the duration of enjoyment is dependent upon the time the animal is permitted to eat. Enjoyment time is limited by the quantity of food available. The findings of Crespi and others can be reinterpreted in terms of the ***duration*** of

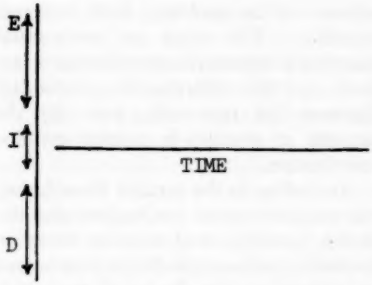

FIG. 2. Frame of reference for representing qualitative, intensive, and temporal changes in affectivity (see Beebe-Center).

affective processes. It is reasonable to assume that the effectiveness of an affective process in organizing a neurobehavioral pattern is in some significant way related to its duration.

Finally, it should be mentioned that there is evidence of a warming-up effect (17). Palatability, under some circumstances, appears to rise slightly before there is a lowering of rate with the approach to satiation.

Our present argument is that analysis in terms of the *kind* and *amount* of food reward is psychologically inadequate unless it takes account also of affectivity. Affective processes are not stable (as are the kind and amount of food) but they change from time to time.

The qualitative, intensive, and temporal aspects of affective processes can be represented within the frame of reference presented in Fig. 2. The vertical at the left represents the affective continuum (1) extending from the most intense enjoyment (*E*) through the range of indifference (*I*) to the most intense distress (*D*). Affective intensity is represented by the distance above or below the horizontal line. Duration is represented in the horizontal dimension. The affective state of an individual varies in different ways. It moves from indifference to distress (increasing distress) or from distress toward indifference (relief from distress). The affective state moves from indifference to enjoyment (increasing enjoyment) or from enjoyment to indifference (decreasing enjoyment). Conceivably a single curve can represent the qualitative, intensive, and temporal changes in affectivity during a given period of time.

Affectivity and the Habit-drive Mechanism

Current theoretical discussions within the framework of stimulus-response psychology assume the existence of (a) innate primary drives, and (b) habits. The position here taken does not imply the existence of pre-formed physiological drives. Instead we assume primary affective processes on the basis of which neurobehavioral mechanisms are organized. The neural mechanisms that regulate goal-oriented behavior, we assume, are always learned.

Miller (8), and others, speak of *learnable drives* or *learned drives*. We assume that there is only one kind of neural mechanism underlying learned drives and habits. The distinction between learned drives and habits is useful but it does not necessarily imply two distinct kinds of neural mechanism.

The terms *habit, learned drive, expectant set,* etc., designate different aspects of neurobehavioral processes. The term *habit* implies that an activity has been developed through learning (practice or training). The term *drive* implies that an instrumental act is motivated in the sense that the underlying neural mechanisms release the physical energy of bodily movement and regulate the pattern of behavior. The term *expectant set* points to the preparatory or anticipatory aspect of the total process.

Primary (unlearned) motivation is of two main kinds. First, stimuli, or physical energies, motivate responses. In the primary drives persistent stimuli originate from tissues in need, from muscular

tension, and from the pain stimulation of injury. Second, hedonic factors also motivate behavior. Distress instigates action to reduce or remove it. Enjoyment instigates action to heighten or prolong it. In this sense affective processes constitute primary motivation. They both initiate and organize patterns of behavior.

Stimulation is not the whole story of primary motivation. When an infant is deprived of water or food or when he is wet and pricked with a pin, he simply cries and screams and kicks at random. Psychologists, as well as parents, assume conscious distress. When the distress is relieved, the infant becomes quiescent. There is a primary motivation to relieve distress. There is also a primary motivation to continue playful, sexual, feeding, and other forms of behavior.

Hedonic theory does not supplant the stimulus-response theory; it is supplementary to that theory of motivation.

Hedonic Factors and Learning

With rats the relation between hedonic factors and learning has been explored experimentally. In one experiment (18) it was found that if the frequency and distribution of trials is held constant, rats *run* faster for a preferred incentive than for a non-preferred incentive at all stages of practice. In fact, the rate of running is directly related to the palatability level of the incentive (as shown by tests of preference). Again, for any test-food that is accepted, the rate of running is dependent upon practice. There are thus two main factors regulating the rate of behavior: affective intensity (degree of enjoyment) and practice. These factors are ordinarily inseparable but they can be shown experimentally to be independently variable.

Again, in tests of preference two foods are presented simultaneously for choice. With repeated trials the percentage of choices of the preferred food increases steadily. The curve of preferential learning is dependent upon the same factors: (a) the difference in *palatability* between the test-foods, and (b) the amount of *practice* in making the discrimination.

According to the present formulation, the acquired neural mechanisms that underlie appetitive and aversive behavior, including preferential discrimination, are organized on the basis of two main principles:

(1) There is a principle of ***hedonic organization***. An individual organizes neurobehavioral patterns that enhance enjoyment and relieve distress. The strength of a determination is directly related to the intensity of enjoyment or (presumably) to the intensity of relief.

(2) There is a principle of *exercise*. When a neurobehavioral pattern is organized hedonically, it is exercised the first time it occurs. On subsequent occasions the same or similar pattern is re-organized hedonically and further exercise occurs. Exercise modifies the neurobehavioral pattern in the sense that it increases its probability of occurrence in the same or similar situation, even when hedonic processes are no longer present.

Hedonic factors are motivating in the sense that the organism seeks to reduce or remove distress and to enhance enjoyment. Affective processes initiate activities that are necessarily exercised as they occur. The affective processes thus play the role of promoting exercise. It is our view that the exercise (whether or not the motivation is hedonic) results in learning according to the widely-recognized principle of contiguity.

It should be made clear that forms of motivation other than hedonic motivation also result in learning. In many experiments upon conditioning, for example, the stimulus-response formula

appears to be quite adequate. Again, when a human subject determines to learn a list of nonsense syllables he can learn them through sheer repetition even though the whole process is affectively indifferent or even unpleasant. In general, we assume that all behavior is motivated and that activity is essential for learning to occur.

In this connection the question can be raised whether hedonic organization itself is a form of learning. This is a matter of definition. Mowrer (9) has formulated a two-factor theory of learning but it seems better to the present writer to define learning in terms of contiguity of exercise, as Guthrie has done, and to regard affective processes as motivating (energy-releasing, organizing) factors in performance.

In any event, our analysis forces a distinction between the hedonic organization of neurobehavioral patterns, on the one hand, and the effects of practice, on the other.

Hedonic organization, we assume, is immediate. There is no retroaction of effect. A pattern is both organized and exercised. Under the total conditions neural organization carries over from one stimulating situation to the next. The learned neural organization is latent from trial to trial; it is the basis of the carrying-over.

Purposive determinations, or motives, are learned. When we speak of a *learned drive* we are referring to the fact that the neural mechanism: (a) liberates energy that initiates an act, (b) regulates the pattern of behavior, and sometimes (c) prepares the organism expectantly for an impending situation. Actually the observed strength of a learned drive is dependent upon both hedonic intensity and practice. Motivation and learning are both aspects of the acquired neural organization.

It is obvious that details of learning theory cannot be considered here but a few words should be added about symbolic rewards. If poker chips bring food to the chimpanzee, the poker chips become adequate incentives. They are a means to an end and they become appreciated for their own sake. To win poker chips becomes a learned motive through association with the primary motivation of enjoying food. The facts indicate that secondary goals become functionally autonomous or independent of the original motivation.

Reward and Punishment in the Light of Hedonic Theory

Stemming from Pavlov's work upon conditioned reflexes, the concept of reinforcement is now well established in theoretical and experimental psychology. Let us examine the concept for a moment.

Reinforcement is actually an ill-defined term. The *New Standard Dictionary* points out that *reinforcement* means strengthening. An army is strengthened by bringing up reinforcements. In the sphere of behavior it is clear that exercise (practice or training) *reinforces* a pattern of behavior in the sense that it increases the rate of its occurrence. This, however, is definitely *not* what psychologists mean when they state that a food incentive reinforces a behavioral pattern. The common view is that reinforcement results from need-reduction or drive-reduction or tension-reduction. The law of effect in some way is entangled with the concept of reinforcement (10).

In a good many experiments a food reward is said to reinforce those activity patterns that lead to it. To clarify the picture from the point of view of hedonic theory let us first consider situations in which the subject is given a reward and then situations in which the subject is punished. Actual experiments upon reward and punishment will not be reviewed here.

An animal is placed in a situation that leads to reward (S_r). After preliminary exploration the animal continues a consummatory response that, we assume, indicates enjoyment. When the rewarding situation is repeated,

$$S_r\ S_r\ S_r\ S_r\ S_r\ \ldots$$

there is repeated or perhaps sustained hedonic arousal. To be specific, a rat is placed in a straight-away leading to food; when he finds the food he continues eating it until the food is consumed or removed by the experimenter. Continued eating, we assume, indicates that the response is affectively positive. According to the first of the above principles some neurobehavioral organization has occurred during the enjoyable eating. Also the consummatory response and the behavior leading up to it have been exercised. Neural organization has been learned.

Next, let us suppose that the reward is removed by the experimenter after it has been consistently present for a number of runs:

$$S_r\ S_r\ S_r\ S_r\ S\ S\ S\ S\ \ldots\ .$$

If running continues after removal of the reward, this fact must be explained in neural terms. Actually, continued running in the absence of a reward leads to extinction of the pattern. The fact that extinction occurs implies that hedonic organization or re-organization is necessary for a neurobehavioral pattern to continue.

This raises a problem for experimental investigation. What, as a fact, actually occurs when the situation is intermittently or occasionally rewarding? In other words, how are hedonic organization and practice interrelated? It is our belief that the experiments dealing with intermittent reward, delay of reward, change or removal of reward, symbolic reward, etc., can be adequately interpreted in terms of hedonic theory.

Now let us examine situations in which the subject is punished. Punishing situations differ among themselves in several ways.

First, the psychological effect of punishment differs with its intensity. In low degree a punishment is instructive, serving to define errors of response. In moderate degree a punishment is incentive. It is something unpleasant to be avoided. In high degree a punishment is disorganizing, disruptive. Clearly, these different effects must be distinguished from each other.

Second, laboratory situations as well as those in real life often punish an animal without providing any means of escape. The psychological effect of punishment depends on whether or not the subject through efforts of his own can escape the distress of punishment.

Third, punishment may be disorganizing rather than organizing. According to hedonic theory it is the *relief* from distress that organizes neurobehavioral patterns and not the distress itself. For the present we are forced to assume that the degree of relief is some function of the degree of distress. The relation between distress and relief, however, can be investigated experimentally.

When an organism is repeatedly placed in a punishment situation, he develops an expectancy of pain or an affect of anxiety, as Mowrer (9) has pointed out, or he develops a fear drive, as Miller (7) has shown. In any event, the facts compel us to add a third principle to the above two:

(3) There is *hedonic disorganization*. If relief is not produced through the activity of the organism, acute or chronic disorganization may occur.

This principle of hedonic disorganization must be checked against the large and expanding literature upon punishment, experimental neurosis, and related topics; but the task is too large to be

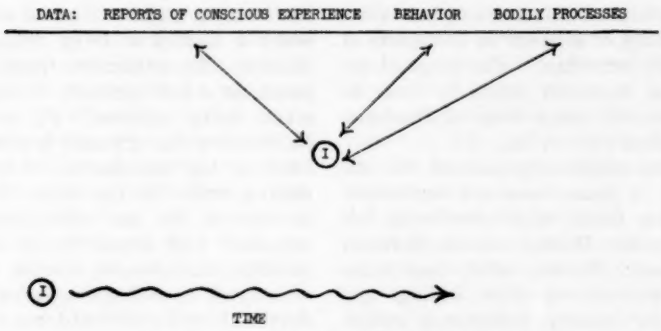

FIG. 3. Fact and theory in psychology.

undertaken here. For the present, let us consider briefly how punishing situations can be analyzed from the point of view of hedonic theory.

To begin with, some situations are punishing and others are not:

$$S_p \; S_p \; S_p \; S \; S \; . \; . \; . \; .$$

If the punishment is disorganizing, the inducing situation minus the punishment might reinstate the signs of disorganized response.

If relief from the distress of punishment is possible, the punishing situation can be represented in this way:

$$S_{p.r}$$

Commonly, the relief is a direct result of the behavior of the subject and according to hedonic theory those patterns of behavior that bring relief are organized, exercised, and hence learned. (It should be noted that the symbol r is used both for *relief* and for *reward*. This might be confusing but it should be remembered that both have similar results in the organization of neurobehavioral patterns.)

Fact and Theory in Affective Psychology

I am sure that when some psychologists read the foregoing discussion they will raise objections: This is old stuff, Thorndike once again! Or, this is a reincarnation of the ancient pleasure-pain theory objectionable because of the implication that psychic causation has physical effects! Or, this is anthropomorphic! It goes Tolman one better by assuming that rats *feel* enjoyment, distress, relief, and that their conscious feelings are efficient causes in behavior!

To meet these objections fairly I will state the main presuppositions of hedonic theory. Most fundamental, perhaps, is the basic distinction between fact and theory. This is the distinction between what is given or observed as datum, on the one hand, and the inferences or assumptions made by the psychologist on the other.

In Fig. 3 the data of psychology are represented above the horizontal line. For convenience three main kinds of data are shown: (1) Reports of conscious experiences of human individuals, (2) The behavior of men and other animals, observed objectively, (3) Bodily processes especially those in receptors, nerves, effectors, observed objectively. Below the line is all that is assumed on the basis of the total data. Psychologists agree in assuming an individual, variously designated as a subject, an organism, a nervous system, a person. These designations do not im-

ply a plurality of individuals but rather a plurality of interests in or aspects of a single individual. The assumed individual, moreover, exists in time; to represent this fact a temporal dimension is required (below, Fig. 3).

I have consistently assumed that the *reports* of pleasantness and unpleasantness are based upon consciously felt experiences. Human subjects do report enjoyment, distress, relief, near satiation, and various other feelings and emotions (anxiety, resentment, amusement, etc.). These terms symbolize genuine data of conscious experience. Affective experience is directly given to individuals as psychological datum; it is not hypothesis so far as the conscious subject is concerned. The terms *enjoyment* (pleasantness) and *distress* (unpleasantness) and *relief*, etc., imply a subjective or individual point of view. This should be kept in mind even though one rejects conscious data as data for science.

Beebe-Center (1), in 1932, defined hedonic tone as an algebraic variable, whose positive values correspond to pleasantness and whose negative values correspond to unpleasantness. For him pleasantness and unpleasantness are *concepts* characterizing experience. This hedonic concept rests in good part upon the verbal reports of human subjects that they are more or less pleased and displeased, and that they have preferences.

When we depart from the reports of conscious feeling for theoretical interpretation we may go directly to a physiological theory as Hebb (4) has done. Hebb has considered affectivity in the context of psychoneural theory. Pleasure, according to Hebb, is not the activity of particular structures in the nervous system but fundamentally a directed growth or development in central organization. "It [pleasure] is thus necessarily a transient state of affairs in which a conflict is being reduced, an incipient disorganization being dissipated, or a new synthesis in assembly action being achieved" (4, p. 232). Hebb's view that pleasure is closely related to the organization of behavior squares well with the facts. It takes account of the fact that pleasure is associated with acquisition of behavior patterns, that pleasure is more marked with newly-formed and growing habits than with well-established habitual activities.

Hebb regards pain as a disruptive somesthetic event but he also considers pain as a specific sensory process. The distinction between unpleasantness (affective) and pain (sensory) is not clearly drawn in Hebb's discussion of pain but the distinction, of course, can be validly made.

To the strict and consistent objectivist I should like to point out that it is unnecessary to introduce subjective concepts into the analysis of behavior. It is fact of observation that a rat accepts one food and rejects another, that the rate of ingestion varies with the concentration and temperature of a solution, that stable food preferences develop with repeated preferential discriminations, that such facts depend upon the constitution of the diet, etc. It is fact of observation that a rat squeals and moves about actively when given a moderate electric shock, that he urinates, defecates, avoids light, etc. Patterns of play, of sexual behavior, of hostile attack, and others, are clearly and definitely observable. Behavior can be described and analyzed without reading into it any subjective concepts. A so-called operational approach to the study of hedonic processes is possible objectively as well as subjectively.

When, however, one comes to the theoretical interpretation of behavioral facts there are several possibilities.

First, there is anthropomorphic interpretation. For example, we are projecting human feeling into the rat's behavior when we say that he *enjoys* one food more than another, or when we regard his squealing and jumping off the charged grill as indicative of *distress*. Anthropomorphic interpretation may help one *understand* the behavior of animals but obviously there is no way for the psychologist to observe directly the conscious experiences of the rat.

With man, however, the situation is different for reported feeling can be correlated directly with objective facts of behavior.

Second, there is interpretation in terms of logical symbols. Hull's theoretical system is the outstanding contemporary example of a logical explanation. Between stimulus and response Hull has postulated intervening variables.

On this level of interpretation I would differ from Hull in two respects: (1) Instead of postulating separate habit and drive factors I would postulate a single determining mechanism that reveals various aspects in behavior (see above). (2) Instead of assuming that reinforcement comes from drive-reduction I would definitely assume hedonic factors determining the initial organization or disorganization of neurobehavioral patterns.

Third, there is physiological interpretation. The facts of behavior can be related to the facts of neurology and general physiology and common interpretations, in terms of a physiological model, can be made. This is straightforward and important.

A still broader theoretical approach, however, is indicated in Fig. 3. Instead of limiting our data to the objective facts of behavior and physiology we take account also of the reports of human conscious experience. In the last analysis all data of psychology—whether they relate to conscious experience or to behavior or to observed neural processes—must be interpreted systematically in terms of processes within an *assumed* psychological individual. The assumed individual has physical properties but also he is an agent with cognitions, desires, emotions.

Just how all the properties of the individual can be brought together into a single concept is something of a problem but it seems to the writer that the biological organism is the best model with which to make the analysis since the organism alone can account for the objective data of physiology and behavior. The concept of the organism, however, must be made fully adequate to explain the facts of conscious experience.

Within this broad theoretical framework we can fill in details of hedonic theory. It is frankly recognized that the concepts of enjoyment and distress (pleasantness, unpleasantness) are based upon human conscious experience but it is argued that they also have objective meaning. An objective psychology of affectivity already exists, although it is in the early stages of development. To the physiologist and physiological psychologist one can ask: What is the neural basis of enjoyment, distress, relief, hunger, satiation, fear, anger, etc.? These questions are straightforward and some day straightforward answers can be given. In the meantime it would be unfortunate if the fear of subjectivism served to obstruct a much-needed development of hedonic theory.

CONCLUSION

The present paper is an attempt to formulate a hedonic theory of neurobehavioral organization. We recognize that current stimulus-response psychology does not adequately take account of central affective processes.

The view that need-reduction or drive-reduction reinforces behavior is abandoned. Instead, three principles are postulated. First, neuro-behavioral patterns are organized hedonically to enhance (heighten, prolong) enjoyment and to relieve distress. Second, a neuro-behavioral pattern that is organized is also exercised and the exercise of an activity leads to learning. Third, distress (especially when intense, persistent, and unrelieved) is disorganizing and underlies emotional and traumatic disturbances.

For any adequate analysis of the organization and disorganization of behavior the concepts of need, affectivity, and motive, must be clearly defined and distinguished.

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"DYNAMIC SYSTEMS" AND THEORY CONSTRUCTION

BY WILLIAM KESSEN AND GREGORY A. KIMBLE

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In three recent articles (1, 2, 3) Professor Krech has presented a brief for the necessity of using neurological concepts in psychology and has proceeded on this ground to develop a quasi-neurological theory pivoted around the concept of "dynamic system." We object to his premise—that psychological concepts *must* be neurological—and to the sort of theorizing to which this conviction leads him. Even more, we oppose the assertion that psychological theory can progress only when we are willing to indulge in neurological speculation. In direct contrast, we hold that there is nothing intrinsically more fruitful in physiological theory than in any other kind; further that what Krech calls purely psychological theory is actually in a stronger position insofar as it remains uncluttered by an anachronistic search for "reality" and "true essences."

Krech's delineation of the pure (or psychological) psychologist is not explicit, but it seems to turn around the notion that such people deal (or should deal) exclusively with observables, never straying far from tough data. But at the same time, Krech quite correctly sees words in the writings of pure psychologists which are not merely names of stimulus-response correlations. He then makes the gratuitous assumption that these words are hypothetical constructs with implied existence. "Ask," he says, "a purely psychological theoretician who proposes to use hypothetical constructs in his theory-making where his constructs . . . are located . . . (2, p. 284). Many pure psychologists do not propose to use hypothetical constructs in Krech's existential sense. A long list of terms which are neither necessarily existential nor simply correlational could be adduced, for which see Spence and Hull, *passim*, or Krech (4). In response to Krech's question, the pure psychologist may quite properly answer that his constructs have no more location than the physicist's concept of force. And he need suffer no guilt or anxiety in making such a reply, so long as the concepts he does use are developed according to the conventional rules of theory building. But Krech apparently does not have this concept of the pure psychologist. In his portrait of him, he says:

> "It now seems to me that a purely psychological approach has had, and still has, tremendous values—but only for one aspect of psychology—the descriptive one" (2, 287).

We do not deny the importance of the descriptive approach. But neither do we believe that the purely psychological approach has "value" for only this one aspect of psychology. The non-correlational, non-descriptive, non-physiological terms in the writings of pure psychologists are theoretical constructs designed to aid in predicting behavior. The extent to which they accomplish this end is the measure of their "value," from which their lack of "neurologicity" subtracts exactly nothing. And much the same argument (in terms of prediction) applies to theory. If Krech's theory turns out to provide for better prediction of behavior than any other, it will be accepted. If it does not, it will be rejected. The issue cannot be prejudged. Psychology (or any science) is a deductive system, the value of which depends upon the level of agreement between prediction and data. Our version of the purely

psychological psychologist is the scientist who erects his theory and develops his concepts so that the deduced theorems can be confirmed or disproved by observations of behavior. This we demand of him, *and nothing more*. The symbols he uses for theoretical manipulation may have any flavor he likes—neurological, physical, sociological, aesthetic—but such a psychologist is not required to specify locus or "real" nature in his theory so long as his concepts mediate the prediction of behavior.

Nor (contrary to Krech) is the pure psychologist inhibited in the range of constructs he may choose. He can invent concepts to the limit of his ingenuity and vocabulary. The purposes of most present-day psychological theorizing are fulfilled by stating the properties which are assigned a concept, usually with the help of a model. Such models, which have the value and disadvantage of all analogical reasoning, may be drawn from any area, without restriction to the biological sciences. Note that Krech takes his dynamic systems model not from neurology, but from the physics of thermodynamics. There is, of course, nothing wrong in this. One of our main theses is that the acceptability of theory is not decided by the kind of model after which it is patterned. We cite the non-neurological character of Krech's dynamic system model, partly to show that Krech, himself, found it either unnecessary or impossible to adhere to his own dictum, and partly to make two further important points. The first of these is that there are many models from which the psychological theorist may choose. In the field of biology, Rafferty (6) lists, *merely as examples*, some twenty-five possibly fruitful models which run from biostatistics through von Bertalanffy's "open systems" to Gestalt psychology. Psychology is in much the same position, and theorists should seek the model which best mediates the prediction of behavior. Other considerations are irrelevant.

A second point is that Krech's use of the dynamic system model makes it difficult to decide just what realm of facts he is trying to systematize. His most recent article (3) sounds very much as if the facts are those of brain physiology. If this is the case, the theory is not even a psychological one. This is a strong attack, and we shall return later to its development, but the basic issue is simply stated. Assume that Krech can demonstrate that his theory does handle our knowledge of brain physiology. He will then have made a great contribution to that field. But the extension of his theory to psychological data will still require a set of co-ordinating definitions stating just what aspects of brain physiology are related to what behavior; further it would be necessary to show that the theory is better applicable than a purely psychological one. We must repeat that the mere claim that a theory is neurological does precisely nothing to give it greater authority than a "purely psychological" approach. For (contrary to Krech) theorizing of the latter kind can be done without confusion, abstruseness or circularity. Indeed, we would argue that these characteristics apply more often to the neurologizing which sometimes goes under the name of physiological psychology. It is in this field that the twin devils, reification and infiltration, romp. Pratt (5) has discussed these dangers in detail. To Pratt's treatment we would add that the faith that our psychological concepts have some sort of material existence seems a vestigial remain of the mind-body controversy.

In introducing his theory, Krech says that ". . . the Dynamic System (or any hypothetical construct in psychology) must be thought of in purely neu-

rological terms . . ." (3, p. 345). At the same time he argues that this does not mean to imply a reductionism; rather that psychology as a ". . . *more inclusive field must enlarge the concepts of the less inclusive if we are ever to achieve a genuine unity of science*" (3, p. 346, italics Krech's).

It is on these methodological considerations that we want to comment first. It may be noted that, by stating that *any* hypothetical construct must be thought of in purely *neurological* terms, Krech has legislated out of psychology all concepts phrased in other physiological terms (e.g., hormonal). This will not endear Professor Krech to his physiologically minded brethren. For it must be pointed out that many kinds of behavior (e.g., sexual) have been explained by theories which emphasize other than neurological aspects of physiology. The substitution of the term "physiological" for "neurological" which would get Krech out of this *particular* difficulty does not appear to be justified on the basis of Krech's theory, which is about "brain fields" and presumably *does* mean to be confined to neurological terms. Or if Krech were willing to restrict even slightly the range of psychological phenomena to which the theory is to apply, his neurological theory might be made to appear more reasonable. But he says quite specifically that his formulation ". . . is to explain all of behavior and all of experience" (3, p. 353). If these statements of ours supporting the physiological psychologist seem to put us in the position of arguing both sides of the case, it is worth emphasizing once more that it is not the particular kind of physiology (or even that it is physiology) that counts. It is the adequacy of description and prediction. It is the absence of this fundamental point in the Krech papers that we regret.

We are not committing the heresy of being against the view of science as unitary, but we do not believe that unity is approached by introducing the sound of neurology in such words as "neural events," "afferent neural interaction" or "irradiation" or the sound of physics with words like "field" or "entropy." Indeed, Krech's position on the question of unified science is quite puzzling. We had supposed that it had been settled once and for all that science is unified in terms of initial data, not in terms of concepts as suggested by Krech in the italicized quotation above. If we gather the implication of this statement, it follows that the anthropologists ought to be busily enlarging the notions of quantum mechanics to embrace the concept of culture. Although science is unified (in the sense we have mentioned), there remains considerable diversity in the kinds of things that different scientists study. What Krech seems to miss is that the concepts of any science are accepted or rejected on the pragmatic criterion of the extent to which they aid in making predictions of *particular* phenomena. Without belaboring an abstruse philosophical question, we reject the imputation that a single true picture of the universe lies hidden by our ignorance, and that there is a royal road to its revelation. If Krech's theory of dynamic systems is to be adopted, it is up to him to demonstrate, not that this is a physiological (or other) *kind* of theory, but that the theory does a better job of summarizing current information in some field, or that it does a better job of prediction, than any set of ideas now available.

The theory which Krech proposes for this purpose turns around the concept of "dynamic system." His most recent paper (3) is an elaboration of these four points: (1) dynamic systems are to be conceived as fields of electrochemical activity; (2) dynamic systems may develop spontaneously toward states of

greater complexity; (3) in its final reorganization, a dynamic system may maintain a steady state and continue to do work; (4) dynamic systems are localized in the brain but conditioned by events in the entire brain. We want to comment, now, not so much upon the potential utility of this set of notions in predicting behavior, as upon these notions as an instance of attempted theory construction.

We may note first that it takes a bit of arguing to justify Krech's claim that this is a neurological theory, but since we have no quarrel with him on these grounds, we shall not pursue the issue.

Second, observe that one searches in vain for a single operational definition. Again, there is nothing *necessarily* wrong with this. All that it tells us is that this is not a theory like that of Hull or Tolman who develop concepts by defining them operationally in terms of manipulable antecedents. But since his theory is not of this sort, Krech seems to have committed himself to the more formal hypothetical-deductive type of theorizing, which demands:

1a. Statement of primitive postulates.

1b. Statement of primitive (undefined) terms.

2a. Derivation (deduction) of theorems.

2b. Statement of derived terms, defined in the postulates.

3. Interpretation of these theorems, that is, translation to some empirical situation.

4. Empirical test of these interpreted theorems.

Insofar as the tests agree with the theorems, the theory is confirmed.

Viewed in the light of this program, Krech's theory could be evaluated somewhat more realistically if we had the following information: (1) *A specification of the various postulates, undefined terms, and so forth.* But all we can be certain of is that Krech's theory contains three postulates about dynamic systems and a limited number of suggested deductions. (2) *An account of the logic by which theorems are deduced from this postulate set.* However, Krech admits that the attributes of dynamic systems may not "follow rigorously from any demonstrably adequate thermodynamic theory of open systems" (3, p. 352). (3) *Definitions which coordinate terms in the theorems with observables.* In Krech's theory this is barely hinted at in his statement that he will "find [the attributes of dynamic systems] useful, later, in dealing with psychological data" (3, p. 352).

It seems that in its present state the theory is (1) not physiological, (2) not operational and (3) not hypothetico-deductive. This almost exhausts the possibilities and raises the question as to just what species of "theory" we are dealing with. The last quotation above seems to supply the clue. On the basis of behavioral data, Krech has made up some rather imprecise concepts, and the system emerges as a crude, but ordinary, R-R theory for which Krech can claim no *special* merit, certainly not on the basis of its alleged relation to physiology.

All of this brings us back of our original objection to Krech's articles, that is to say, to his assumption that the physiological approach is essential to psychological theory. If psychology has the same goals as other sciences, this is not so. All that is essential is that the theory lead by elaboration to theorems which may be tested by observations of behavior. From this viewpoint, the concept of superego has as

sound possibilities for the integration of behavior as the concept of brain fields or dynamic systems. Fortunately, the restrictions which Krech puts upon psychology are not critical. If physiologists are never able to discover the correlates of our concepts, the concepts are still good for whatever they were good for in the first place. If they are discovered to have physiological existence (that is, confirmation in a physiological theory), this is still no guarantee that they will have any greater predictive value than they would have had without this added confirmation. This is an empirical matter. Furthermore, if we were to rule out of science all concepts which have no existential verification, we should have to do without atom, culture, hypotheses, force and many others, including dynamic systems. To this let us add that Krech's own speculations are evidence against his insistence on neurological theorizing. The cortical clang of the theory does not change its status as an attempted application of thermodynamics. Nor does it give it any peculiar strength. When and if neurological notions increase the predictive value of psychological theorizing, they will be used. In the meantime, discretion seems the better part of theory-making.

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A NOTE ON "'DYNAMIC SYSTEMS' AND THEORY CONSTRUCTION"

BY DAVID KRECH

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I fully appreciate the courtesy of Drs. Kessen and Kimble in making their manuscript available to me for my comments. After reading their paper I have the feeling that their manuscript together with my three papers to which their article is addressed make a rather closed circle. Occasionally it even seems that we are taking different positions on the same points—i.e., *some* issues seem to be joined. That is the most we can expect of any discussion. Both sides seem to be adequately represented and there is little that I can add. To those psychologists who are at all interested in the notions I originally proposed I can only suggest that Krech and Kessen and Kimble be read as a point of departure for further cogitation on the matter. I should like to add one point, however.

I cannot agree with the sound and sane dictum expressed by Kessen and Kimble when they say, "If Krech's theory turns out to provide for better prediction of behavior than any other, it will be accepted. If it does not, it will be rejected. The issue cannot be prejudged." Of course I do not find fault with the first two sentences. The logic there seems to me to be admirable. But I disagree with the assertion that the "issue cannot be prejudged." The issue *must* be prejudged. We cannot wait for a proof of any theory before we decide whether or not we shall invest our time and our substance in exploring the theory. Science may have an infinite time for its work and its judgments, but scientists are finite. Any one of us is always prejudging the value of his work, is always making bets on whether or not the particular road upon which he is embarking is the best of all possible roads available. The proof of the pudding may be in the eating, but I suggest that there would be no pudding to eat if we did not have enough pre-eating faith to go ahead and make the pudding. It is because of this that I believe a serious and perhaps even partisan discussion of proposed theories (long before their soundness has been demonstrated) is a fine and good and heuristic thing to do. And it is also because of this, therefore, that I am very glad to see Kessen and Kimble's vigorous discussion of my notions. They are prejudging my "theory," and so they should.

[MS. received August 1, 1951]

"WHAT IS LEARNED?"—A THEORETICAL BLIND ALLEY[1]

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Introduction

The pursuit of knowledge frequently has been hampered by misdirection. Questions lacking any possibility of being answered experimentally are often considered to be empirically meaningful. In many instances this inherent lack of meaning is so well hidden that much time and effort is wasted on attempted answers. The history of psychology has contained many such cases of "meaningless questions"; the search for the basic elements of consciousness and the attempt to estimate whether intelligence is determined by hereditary or environmental variables are but two of the more outstanding examples of such unanswerable questions.

The history of these questions appears to follow a common course of development. Initially the question provokes much interest and seemingly opposed speculations are offered as possible answers. Then the question is subjected to experimental attack. This usually intensifies the theoretical differences because much bickering is instigated about the adequacy of the experimental design as well as the correct interpretation of the experimental data. The result is that agreement is never attained. Gradually the interest in the question subsides and successive generations of scientists learn to avoid the question because they intuitively recognize the futility of pursuing it. Then, finally, the question's inherent lack of meaning is fully realized and exposed.

The recognition of the status of such unanswerable questions need not proceed in the above described haphazard manner with its consequent costliness in time and effort. Present-day philosophy of science has been concerned with establishing criteria for distinguishing between meaningful and meaningless questions. By the application of methodological analysis it is possible to demonstrate that certain problems are not resolvable, not because they are too profound, but rather because the questions they raise cannot properly be answered. It will be the major intent of this paper to demonstrate that the current controversial question of "what is learned" falls into the category of questions that cannot properly be answered.

The Question

The frequency with which learning theorists attempt an answer to the question of "what is learned" has led contemporary psychologists to invest this question with a conspicuous import. In fact, it is implied by such seemingly antithetical theorists as Tolman (15) and Guthrie (5) that *this is the crucial question* confronting the learning theorist; other controversial issues presumably stem from divergent conceptions of what is learned. For example, the current issue of latent learning appears to have its roots in the question of whether cognitive maps or stimulus-response associations are learned.[2]

[1] This paper represents a revision and extension of a lecture presented to the Psychology Colloquium at Brown University and the Psychology Section of the New York Academy of Sciences. The author is indebted to Drs. Tracy S. Kendler, Lyle H. Lanier and Benton J. Underwood for their very helpful criticisms and suggestions.

[2] Exception might be taken to this statement on the basis that the latent learning experiments were conceived in terms of the theo-

Experimental findings, however, appear incapable of resolving these opposed viewpoints. For example, Thorndike (12) in 1898, Muenzinger (10) in 1928, Adams (1) in 1929, and Guthrie and Horton (6) in 1946 all report behavior of animals in a problem box situation. It is interesting to note that their respective answers to the question "what is learned" appear to be at variance. Thorndike, and Guthrie and Horton, concluded that stimulus-response associations were learned, although disagreeing, to some extent, about the conceptual properties of the association. On the other hand, Muenzinger and Adams interpret their data to support the conception that "ideas" or "meanings"—concepts which appear to be equivalent to the more modern "cognitive map"—were acquired. This difference between these conclusions would not be too surprising if disparate findings were obtained. This was, however, not the case. Careful readings of these experimental reports suggest that these experimenters were observing similar behavior patterns of their subjects. Guthrie and Horton write, ". . . Adams' report is so conscientious that we are able to identify in our experiment all the behavior that Adams describes. . . ." One may conclude that if comparable data are employed to support diverse answers to the same question, then the major source of difficulty lies not in the seemingly opposed answers but, rather, in the question itself. An analysis of the question is therefore in order.

retical problem of whether or not reward was necessary for learning. Tolman (15) has recently stated quite explicitly that the results of the latent learning studies are just as inconsistent with an *S-R non-reinforcement* theory as with an *S-R* reinforcement formulation. The major contribution of these experiments, according to Tolman, is that they demonstrate that cognitive maps, rather than stimulus-response associations are learned.

AN ANALYSIS OF THE QUESTION

How do these conflicting conceptions of what is learned arise? At first glance it would appear that they are at the very core of the various theoretical conceptions. While Tolman (15) insists that cognitive maps are learned, Guthrie (5) is equally adamant that what is learned is an association between "some stimulation of sense organs and a corresponding muscular contraction or glandular secretion."

The fact that these conceptions are considered by most theorists and students in the field of learning to be both basic and crucial should not permit us to accept their relevance without question. No matter how deeply imbedded they appear to be in their respective theoretical structures, the possibility remains that these various conceptions of what is learned may be essentially external to the theory. For example, in theoretical physics, mechanical models serve as convenient tools of thought, but they are neither fundamental nor even necessary to the theoretical formulation.

Whether or not a conception of what is learned is basic to a particular learning theory can be determined only by understanding the structural requirements of a learning theory. It may appear strange to use Tolman's blueprint (13, 14) for theory construction as a guidepost in our methodological analysis. His early papers and the more recent "Cognitive maps in animals and men" stand toward one another as clashing antinomies. Tolman has violated methodological requirements which he himself promulgated. Much of the confusion surrounding the problem of what is learned would disappear if learning theorists, especially Tolman himself, would be more sensitive and sympathetic to the implications of these early papers (13, 14). A brief exposition of the structural requirements of a learning theory is, therefore, in order.

Tolman has expounded the pragmatic use of the intervening variable to bridge the gap existing between the independent and dependent variables. Rather than treat separately the relationship each independent variable bears to the many dependent variables, Tolman proposed the grouping of certain independent variables; these groupings or component functions would then be connected logically to constructed intervening variables; these in turn would be connected to one another, and finally to the dependent variables. This *intellectual construction* has as its aims the economic description of the known empirical relationships, and the prediction of new phenomena. Tolman's blueprint for theory construction has been used by himself, Hull (8) and others in their theorizing. The core of these theories has been the intervening variables, e.g., habit strength, cognitive map, etc. Basic to the entire question of what is learned is the specific interpretation given to these intervening variables (theoretical constructs). We shall see that secondary, and unnecessary, elaborations about the meaning of these intervening variables have led to what some have considered to be conflicting views as to what is learned.

There would be no confusion about the meaning of such terms if it were always remembered that these intervening variables serve as *economical devices* to order experimental variables in relation to the dependent variables. They are "*shorthand*" *descriptions*, and nothing more, of the influence on behavior of several independent variables. The *only meaning* possessed by these intervening variables is their relationship to both the independent and dependent variables. Because this point has been ignored or misunderstood, an immense amount of confusion concerning the "real meaning" of these intervening variables exists.

A somewhat similar situation existed in theoretical physics with regard to the gravitational action between distant bodies. For many physicists the idea of action at a distance was odious; it did not appear conceivable that a phenomenon could be "caused" by a relationship. In order, therefore, to make the phenomenon more "meaningful," different kinds of space-filling mediums capable of transmitting forces from one body to another were hypothesized. But there is no justification, as Bridgman (3) points out, "for the attitude which refuses on purely *a priori* grounds to accept action at a distance as a possible axiom or ultimate of explanation."

Nagel uses a neat example to describe the above type of error which has been labelled the fallacy of reification or hypostatization. He writes:

"Suppose, for example, that Smith invites to his home his foreign friend Forgeron, so that the latter may see for himself what family life is like in America. But suppose that after enjoying Smith's hospitality for some time Forgeron was to complain that though he met Smith's wife and children, had meals in common with them, took part in their recreations, and so on, he had never been introduced to Smith's Family Life. It is clear in this case just what is the nature of Forgeron's error: he mistakenly assumes that "Family Life" is the name of some supra-sensible "entity," distinct from the complex activities in which Smith and the members of his family are normally engaged" (11, p. 236).

In the very same sense the construct of learning, whether it be conceived in terms of modifications in cognitive maps or *S–R* connections, does not refer to an object, thing or entity as suggested by those who are concerned with the question of what is learned. These intervening variables possess no meaning over and above their stated relationships between the independent and the dependent variables. The basic error underly-

ing the problem of what is learned is the assumption that these intervening variables are entities capable of being described and elaborated upon, independent of their operational meaning. The fallacy of reification yields the problem of what is learned. The realization that learning is not a "supra-sensible entity" disposes of it.

INTUITIVE MODELS VS. OPERATIONAL MEANING

It would be interesting, and perhaps fruitful also, to go beyond our initial methodological analysis in order to understand the psychological factors underlying the confusion surrounding the question under consideration. After all, psychological theorizing is just another form of behavior. Why cannot we attempt as psychologists to understand why meaningless questions are asked and concepts are reified?

It appears to this writer that the confusion surrounding the meaning of theoretical constructs stems from the failure to distinguish sharply between personal thought processes leading to the invention of theoretical constructs and the *operational meaning of the term*. The first is a problem in the psychology of creative thinking (in this case the psychology of the psychologist), the second is a problem in epistemology. Why Hull (8), for example, chose to define habit strength in the manner he did is a function of his intellectual abilities, his style of thinking, his personality, his interests, etc. In order to know what habit strength is, one need not know the intimate details of Hull's background or have a knowledge of his intellectual functioning; one need only know how this concept is tied down to "observable events," i.e., its operational definition. The thought processes of the psychological theorist may, as they usually do, involve the use of some sort of a model. It becomes necessary, when such models are used as *thinking aids*, that the model not be confused with the scientific concept stemming from it. Too often learning psychologists have been misled into believing that the intuitive models which they associate with their intervening variables possess existential properties above and beyond the operational definition of the intervening variable.

Although psychologists are very willing to admit the existence of individual differences in motor skills, aptitudes, and many other behavior characteristics, they appear markedly reluctant to admit that such differences may exist in creative thinking. Would it be valid to say one must think "phenomenologically" as does Tolman, or within a Darwinian framework, as Hull appears to do, or "algebraically" as does Spence, or in terms of physical models as does Köhler? It is granted that only a valid and extensive knowledge of the psychology of thinking could answer this question. If we refuse to ascribe certain unique and mystical qualities to the process of creative thinking, i.e., if we consider it just as another form of behavior, then we may extrapolate from our knowledge of other behavior. We know that people learn mazes in different manners, runners sprint with different styles, and, if we are to believe Kinsey, people woo in different fashions. Certainly within our limited knowledge of the variables involved in productive thinking, it would appear questionable to assume that only one style of thinking would be fecund; and it would be hazardous, as well as somewhat presumptuous, for any theorist to insist that every other theorist think in his style.

The above point obviously does not imply that theorizing is purely a matter of personal taste. At some stage in the development of a theory, the theorist must meet the requirements of his scientific audience, i.e., the operational definitions of his theoretical (intellectual)

constructs must be made clear to everyone who is sincerely interested in them. This does not mean that the theorist must reveal his private cerebration (for purposes of clarity, it may be best if he did not), but that he must state the relations which his theoretical constructs bear to the observable variables so that their "validity" may be tested.

In evaluating the "validity" of a learning theory it should be recognized that the most we can expect is that the theoretical estimates of the dependent variable be in agreement with the observed responses in an experimental situation. All learning theories must fulfill this function, i.e., generate valid estimates of the to-be-observed response. In this sense *all learning theories are response theories.*

The above analysis leads the writer to conclude that Tolman's recent division between place learning as contrasted with response learning is not sufficiently cogent; it suggests a qualitative distinction where none exists. Such a distinction arises only if one considers differences in intuitive models to be basic to theoretical differences—a position to which this paper would take exception. Using the same sort of logic, one would also be forced to conclude that Guthrie's conviction that movements are learned or Hull's belief that receptor-effector connections are required are as invalid as Tolman's feelings that cognitive maps are learned.[3]

Our initial point was that the pseudo-problem of what is learned emerged from the methodological error of considering learning an entity rather than a process. Our second point is that this methodological error is due to the contamination of the operational meaning of theoretical constructs by their intuitive properties. This contamination has led to spurious formulations of theoretical differences. Rather than persistently pursue both the obvious and subtle connotative meanings of the words used in theorizing, it would be much more productive to attempt to relate the constructs systematically to the manipulable variables and behavior measures. Only then can the explanatory power, inherent in the concepts, be tested.

THE "CAUSE" OF THE CONFUSION

Why have learning theorists indulged so much in unnecessary elaborations of their theoretical constructs at the ex-

[3] In actual practice there are two differences which appear to separate the cognitive theorists as a group from the *S-R* theorists. Firstly, the two appear to possess somewhat different thinking styles. Aside from certain scientific requirements, the selection of the specific terms in a theoretical structure seems to be determined by the personal needs of the theorist. Perceptual and cognitive terms appear to be intuitively satisfying to the cognitive theorist while they tend to evoke suspicion among *S-R* theorists. I suspect that if Hull had labelled what he now calls habit strength "dynamic cognitive field expectancies" without modifying its postulated relationships to the independent variables, much of the opposition to this concept would disappear and probably some new opposition would arise from certain quarters. The second difference is related to the problem as to what point in the "theoretical bridge" between the independent and dependent variables should the nature of the response be indicated. The *S-R* theorist usually specifies the nature of the response relatively early, while the cognitive theorist does not specify the nature of the response until rather late in his theorizing. In fact, they have been frequently criticized, with some justification, because they do not always translate the cognitive map of the organism into specific action; in other words, they do not always bridge the gap between the independent and dependent variables. The point on the "theoretical bridge" at which the initial response estimate is introduced may prove to be an important stratagem in theory construction. Since I have emphasized the operational meaning of concepts rather than their connotative meaning, and because so few contemporary theorists have come to grips with the problem of the location of the initial response estimate, it is felt that the above two differences are not *basic.*

pense of determining their relationships to the observable variables, i.e., the substitutions of models for operational definitions? It is the writer's belief that the answer to this question lies in different conceptions of the nature of scientific explanation.

Again, it may be appropriate to interject into our discussion certain speculations concerning the psychological basis of misconceptions of scientific explanation. Most, if not all, psychologists were confronted with the question "Why?" prior to their exposure to scientific training. It is safe to assume that intuitively satisfying "explanations" were achieved during this pre-scientific era of personal development. There is a strong suggestion that some of these pre-scientific explanatory techniques have lingered on and have been confused with scientific explanation.

Feigl (4), in a brief perspicuous exposition, states that the demand for explanation "is answered by deductions either from empirical laws or from theories." He enlarges upon this dictum as follows:

> "Deduction from empirical laws may be styled 'low grade' explanation. It merely puts the fact to be explained into a class of facts characterized by the same empirical law. Thus the explanation for the fact, e.g., that there is a mirror image of a bridge in a river, is achieved by subsuming this fact under the law of reflection in geometrical optics. This law is simply the common denominator of all the various phenomena in which light-reflection is the essential feature. A 'higher-grade' explanation we find in the Maxwell-electromagnetic wave theory, which serves as a basis for deduction for a variety of optical phenomena; reflection as well as refraction, diffraction, interference, dispersion, polarization, etc. etc." (4, p. 286).

Psychology has achieved only a few "low-grade explanations." For example, Hull was able to deduce from his initial goal gradient hypothesis the locomotion gradient of rats in a runway, blind alley elimination, the more rapid elimination of long blind alleys as compared to short blind alleys, the backward order of elimination of blind alleys, and similar phenomena. In a much less formal manner Freud has attempted to integrate such phenomena as slips of the tongue, dreams, hysterical symptoms, tics, etc., by assuming certain unconscious mental processes.

This requirement for a deductive component has too often been ignored in attempts at explanation. The reason for ignoring the requirement of a deductive component in scientific explanation stems from the failure of some writers to distinguish clearly the requirements of scientific explanation from those attributes of propositions which are capable of instigating in some a feeling of "understanding" (the "a-ha" phenomenon). The contention of this paper is that some of the arguments surrounding the question of what is learned have stemmed from such confusion, i.e., between scientific explanation and, for want of a better term, "psychological understanding." This sort of confusion has been particularly noticeable in many of the formulations involving physiological and/or phenomenological terms.

A common misconception among some psychologists concerned with discovering the physiological correlates of behavior is that their area of interest excuses them from the task of developing a theoretical system capable of generating valid deductions. Believing that they are dealing with the "real causes" of behavior,[4] they feel that the mere specification of the physiological factors or, more com-

[4] It has always been a mystery to me why, if one desires to be entrapped by the metaphysical problem of "real causality," one should be satisfied to stop at the physiological level without descending into the physicochemical, or the atomic, or the sub-atomic level.

monly, the mere speculation concerning the physiological processes is sufficiently explanatory. For example, Birch and Bitterman (2), in a recent polemic against *S–R* reinforcement theory, conclude that it is *necessary* to postulate a physiological process of sensory integration to explain conditioning. This conclusion is reached in spite of their admission that ". . . we know very little about the conditions under which sensory integration occurs." Actually, they do not specify any physiological variables or behavior measures to which "sensory integration" is connected. The result is that their formulation is incapable of generating any deductive implications—and consequently is void of any explanatory ability. Adequate explanatory systems in physiological psychology, as well as in other areas of psychology, must have a deductive component.

The above should not be interpreted as an attempt to make an invidious comparison between behavioral and physiological theories. The development of psychology can and should be furthered by serious and rigorous attempts at formulations of both behavioral and physiological theories. This writer believes (on an intuitive basis) that, at the present time, the program of independent development at both levels would be the most strategic, followed, of course, by an attempt at coordination. Another procedure would be to develop behavioral and physiological formulations interdependently and simultaneously. There is no reason why such attempts could not be successful. The tendency, however, has been merely to use some physiological-sounding terms, extracted in all likelihood from the private thought processes of the theorist, without specifying their relations (hypothetical or established) to the experimental variables and behavior measures. Although such a procedure provides a ready "answer" to the question under consideration ("what is learned"), these answers contribute nothing to our understanding of the learning process.[5]

A similar problem is raised by some theories which use phenomenological terms as labels for their theoretical constructs. It appears that for many learn-

[5] The problem of the physiological properties of intervening variables has been raised in a somewhat different form by MacCorquodale and Meehl (9). They argue that it is important to distinguish between two types of theoretical constructs, hypothetical constructs and intervening variables. The former "involve the hypothesization of an *entity*, *process*, or *event* which is not itself observed . . ." while the latter ". . . do not involve such hypothesization." These writers demand ". . . of a theory of learning that those elements which are hypothetical . . . have some probability of being in correspondence with the actual events underlying the behavior phenomenon. . . ." Referring to some of Hull's early theoretical papers in this JOURNAL, MacCorquodale and Meehl write:

"We suspect that Professor Hull himself was motivated to write these articles because he considered that the hypothetical events represented in his diagrams may have actually *occurred* and that the occurrence of these events represents the underlying truth about the learning phenomena he dealt with" (9, p. 104–105).

Here again, the personal cogitations of the theorists are confused with the operational meaning of the concept. Although it may appear "clinically justified" to assume that Hull had some conceptions of physiological mechanisms coordinated to some of his theoretical constructs, the meaning of those constructs should be confined to their operational definition. Other theorists who work within the *S-R* reinforcement theoretical framework have no such intuitive physiological conceptions. Should we therefore have two (or possibly more) conceptions of these physiological sounding concepts, the constructs varying not in terms of their operational meanings but rather in terms of the individual's intuitive conception? For purposes of communication, it appears wise to separate all the private cogitations from the operational meaning of concepts. The important point is whether the behavioral theory can be divorced from the "physiologizing" without damage to the deductive capacity of the theory. In the case of Hull's theory it would certainly seem possible.

ing psychologists it is essential that their theoretical constructs be phenomenologically consistent, i.e., capture the quality of naive introspective experience. There is little doubt that the theoretical constructs used by psychological field theorists reflect "inner experience" in a more satisfying manner than do associationistic constructs. There is no basic methodological objection (or virtue) to the use of either set of terms *as long as they are related to observables, and as long as they are used in formulations with deductive capacities.* This rule, however, is not always followed. All too frequently, constructs by themselves (e.g., structuring, insight), which describe vividly phenomenological experience, are presented as scientific explanation.

I am convinced that the selection of any theoretical model, be it physiological or phenomenological, or for that matter, physical, mechanical or statistical, is in the last analysis a decision having *no truth character.* That is, in spite of the fact that the choice of a model may, and usually does, influence both experimentation and theorizing, *the choice itself* cannot be evaluated as being right or wrong. It is a matter purely of personal taste. The most we can do is to attempt, in a sincere and conscientious manner, to understand the implications of such decisions, but we should not be led astray by believing we can experimentally test their validity.

What then should the learning theorist do? Instead of seeking an answer to the question of what is learned in a manner based upon inadequate conceptions of scientific meaning and scientific explanation, it would be better for learning theorists to come to grips with their undertaking in a positive forthright fashion, i.e., to anchor their theoretical concepts to observables and unhesitatingly test the explanatory capacities of their formulations.

By means of such a constructive approach, the time wasted in needless arguments of the sort which has centered around the sterile question of what is learned would be reduced and, perhaps, even be eliminated.

SUMMARY

Contemporary learning theorists offer numerous and seemingly opposed answers to the question as to what is learned, the major difference among them being whether "cognitive maps" or "stimulus-response associations" are learned. The argument in this paper is that the problem of what is learned is a pseudo-problem. It stems from the methodological error of reifying theoretical constructs. This reification in turn is due to the failure to distinguish sharply and consistently between the operational meaning of intervening variables and the intuitive properties ascribed to these concepts. If we conceive the intervening variable as being an economical device by which experimental variables are ordered in relation to behavior variables, then this confusion will not arise. The basic difference between such intervening variables as "habit" and "cognitive map" can be specified only in terms of their stated relationships to the observable variables, not in terms of the connotations they arouse.

At the bottom of this seeming disagreement as to what is learned is the failure of many psychologists to utilize an adequate criterion of scientific explanation. All too often, propositions (usually involving phenomenological or physiological terms) which have no deductive capacities are accepted as explanations because they instigate in some a sense of "psychological understanding." It is proposed that learning theorists avoid the problem of what is learned and come to grips with their undertaking in a positive forthright fashion. This can be accomplished by

relating theoretical constructs to observables and unhesitatingly testing the explanatory capacities of their formulations.

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A UNIFYING THEORETICAL APPROACH TO MOTOR LEARNING [1]

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INTRODUCTION

Within the last fifteen years there has developed an extensive literature in the field of motor learning. A great many of the investigations have been undertaken in order to formulate a "new" theory of motor learning, while others have been designed to test one or more of the already existing theories. As a result of the differing theoretical orientations and the frequency of apparently divergent experimental results, it was almost inevitable that there would develop an increasingly insistent need for a theoretical approach to the problem that would be generally applicable to a wide range of studies.

An attempt will be made in this paper to indicate a possible way in which a recent theoretical approach may be employed to explain and thereby give added meaning to the results of several investigations which are quite divergent when considered in terms of their respective original theoretical explanations. This attempt is being made here in the interest of a more generalized theory of learning. However, it should be emphasized that the interpretations to be offered in this paper should be taken as suggestive of further research, and not as a final formulation.

The present theory is based upon Hull's (7) Performance Inhibition Theory and the more recent extension of this theory advanced by Kimble (10), some of whose formulations have been substantiated and extended by Wasserman (16). Kimble's (10) modification of Hull's theory may be summarized as follows:

The total inhibitory potential is made up of two elementary components, reactive inhibition (I_R) and conditioned inhibition ($_sI_R$). Reactive inhibition is a drive state similar to pain avoidance. It builds up as a positive function of the amount of work involved in the response in question, and dissipates during a rest interval. In massed practice, I_R is not allowed to dissipate, and therefore the need for rest increases as greater amounts of I_R accumulate. When resting occurs, I_R dissipates and this serves as drive reduction or reinforcement. Because of this reinforcement, the response of resting will become conditioned to whatever stimuli are present in the learning situation. This resting tendency is then a habit, and is called conditioned inhibition.

Starting with this basic part of Hull's theory, Kimble (10) hypothesized that with continuous practice the accumulation of a certain amount of I_R will produce an automatic resting response, thus resulting in the dissipation of I_R before a formal rest interval is introduced. Once I_R is reduced below the critical point, work is resumed and continues until the critical level of I_R is reached again. Thus, a kind of equilibrium is established which results in the maintenance of I_R at, or slightly below the critical level. It should be noted that motivation is of crucial importance in

[1] This paper constitutes a portion of a dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, in the Department of Psychology in the New York University Graduate School of Arts and Science.

[2] All of the work connected with this paper was completed while the writer was at New York University.

this process, and should it decrease during learning, the critical level of I_R will drop somewhat.

Kimble further hypothesized that the development of ${}_sI_R$ does not begin until a threshold amount of I_R has accumulated. That is, no ${}_sI_R$ should be present very early in learning.

Finally, Kimble hypothesized that any condition which tends to reduce the critical amount of I_R will increase the amount of resting, and therefore, the rate of development of ${}_sI_R$. Thus, if motivation should decrease late in learning (as often occurs with some activities), the rate of development of ${}_sI_R$ should increase late in learning.

The results of two studies by Kimble (10, 11) substantiated these three hypotheses, and another study by Wasserman (16), which dealt with the factor of motivation more explicitly, also confirmed these points and indicated that high motivation serves to raise the critical level of I_R necessary to produce the automatic resting response.

With this brief description of the present theory in mind, it will now be worthwhile to survey some of the existing theoretical approaches to the study of motor learning in order to make some critical comparisons.

EXISTING THEORIES

A recent article by Ammons (1) may be used as a point of departure in this survey. Ammons offers a theoretical formulation, the constructs of which are very similar to those of Hull even though different symbols are utilized. However, of more interest here is Ammons' review of the extent to which existing theories can account for the observed characteristics of a motor learning curve *after the introduction of a rest*. These characteristics are as follows:

(1) An abrupt rise in the curve.

(2) A gradual flattening out of the curve.

(3) Resumption of the pre-rest curve of gradual improvement.

(4) Permanent difference in performance level.

Ammons summarizes the adequacy of each of five different theories to account for these observed characteristics. The theories dealt with are as follows: (a) The Primary and Secondary Growth Theory of Snoddy; (b) The Maturation Hypothesis of Dore and Hilgard; (c) The Learning and Work Factor Theory of Dore and Hilgard; (d) The Interference and Warming Up Effect Theory of Bell; and (e) The Performance Inhibition Theory of Hull.

A. *Hull's Performance Inhibition Theory.* Though Ammons deals with Hull's theory last, it will be more advantageous here to deal with it first since the present theoretical approach is directly based upon it.

Of all the characteristics of the post-rest motor learning curve which were listed above, the last one (permanent difference in performance level) is the most difficult to explain. In fact, of all the theories reviewed by Ammons (1) and in the present survey, only Hull's inhibition theory can account for the permanent difference produced by the introduction of a rest interval. According to this theory, this permanent decrement which is characteristic of massed learning as compared with spaced learning is attributed to the greater amount of conditioned inhibition which is built up with massing. Since ${}_sI_R$ is a habit and does not dissipate with time, its effects are relatively permanent, thus, the superiority of spaced learning is understandable.

Ammons states that Hull's theory does not directly account for the first three characteristics of the post-rest motor learning curve. However, these characteristics may be explained in terms of the present extension of Hull's theory as follows:

(1) The abrupt rise is understandable in terms of the dissipation of I_R during the rest interval.

(2) The gradual flattening out might be understandable in terms of the building up again of inhibitory potential, and the influence of the warming up effect. The latter is a decrement brought about by the loss of certain perceptual-motor adjustments during the rest interval. This decrement often does not show up immediately after rest because of the greater opposite effect of the dissipation of I_R which occurred during the rest interval.

(3) The resumption of the pre-rest curve of gradual improvement might be explained by utilizing some additional concepts of Hull's theory. As noted above, a kind of equilibrium is set up which maintains I_R at, or slightly below the critical point. It is conceivable that with continuing motivation and reinforcement, the reaction potention ($_sE_R$) will be greater than the total inhibitory potential ($\dot{I}_R$), and thus, the effective reaction potential ($_s\bar{E}_R$) will be large enough to produce gradual improvement ($_s\bar{E}_R = (_sE_R - \dot{I}_R)$) (7, p. 284). In addition, it has been found by Ammons (2) and Irion (9) that the warming-up effect reaches a maximum after the first few trials following rest, and then levels off. In other words, after the first few post-rest trials, there is a reinstatement of set to perform the task. Thus, with continuing motivation and reinforcement the characteristic gradual improvement is understandable in terms of the concepts of effective reaction potential and reinstatement of set.

B. *Snoddy's Growth Theory.* According to Snoddy (13), there are two opposed processes in mental growth. One of these he calls primary growth and the other secondary growth. Primary growth occurs early in learning, is relatively stable (resists loss with time), and is a positive function of the number of repetitions and the amount of time interpolated between trials. As the amount of interpolated time increases indefinitely, the amount of primary growth will increase indefinitely.

Secondary growth occurs later in learning, is relatively unstable (is lost over a long period of time), and is a positive function of the amount of primary growth and a negative function of the length of interpolated time. Secondary growth is at a maximum when the interpolated time is zero, that is to say, when practice is massed.

In order to test this theory, Humphreys (8) performed an experiment with the pursuit rotor, and found that the greater gains early in learning occurred *between* practice periods (after a forty-eight hour rest), while in the later stages of learning the gains were made *during* the practice periods.

Similarly, Travis (14), utilizing six-minute work periods separated by intervals of three and seven days with rotary pursuit learning, found that gains *between* practice periods were confined to the early stages of learning.

The results of both of these experiments tend to support Snoddy's theory. However, another experiment conducted by Travis (15) yielded results inconsistent with it. Using the pursuit-oscillator with rest intervals ranging from five minutes to one hundred and twenty hours, Travis found that, contrary to the theory, learning (primary growth) was not increasingly better as the length of interpolated time increased. The optimum rest was twenty minutes, and in addition, the shortest rest (five minutes) resulted in better learning than the three longest intervals (48, 72, and 120 hours). According to Snoddy's theory of primary growth, the longest rest interval should have been the most beneficial.

A basic difficulty with the theory is

the way in which Snoddy attempts to explain observed decrements in performance. The explanation is in terms of interference between the two growth processes. The nature of this interference is not at all adequately elaborated.

It seems possible to explain all of these results in terms of the present inhibition theory. Decrements in performance are most easily explained in terms of the accumulation of inhibitory potential. In addition, the gains *between* practice periods are adequately explained in terms of the dissipation of reactive inhibition during the rest interval. The fact that Humphreys and Travis found the gains that occurred between practice periods (after a rest) to be confined largely to the early stages of learning is also understandable without need of Snoddy's theory. Reactive inhibition is built up rapidly and reaches the critical level quite early in learning. However, late in learning, when motivation decreases, more resting occurs during learning and this critical level will tend to decrease. Hence, later in learning less gains are made *between* practice periods because there is less reactive inhibition present to dissipate when a formal rest interval is introduced. This observation that increases after rest are not as great late in learning as they are early in learning has been confirmed by several more recent studies (2, 9, 10).

Furthermore, it should be noted that when a rest is introduced, almost all the reactive inhibition is dissipated during the first few minutes of rest (with pursuit rotor learning). In fact, Ammons (2) found that there is probably no further decrease in reactive inhibition after about twenty minutes of rest. However, Humphreys used 48-hour intervals and Travis used three and seven day intervals. Under these conditions, if any increase at all is found after the interval, it would be expected in the early stages of learning when motivational factors are at a maximum, and such factors as warming-up effect and retroactive inhibition are outweighed by the beneficial effects of the dissipation of reactive inhibition. Thus, the effect of the dissipation of reactive inhibition is indicated by a gain in score after rest early in learning, while late in learning the beneficial effect of the dissipation of reactive inhibition *may* be outweighed by such influences as loss of set (warming-up effect), and retroactive inhibition, the effects of which may be enhanced by a drop in motivation which often occurs later in learning with this type of task.

The results of Travis' second experiment (15) are also in agreement with the present theory. Several studies have shown that the longest rest interval is usually not the most advantageous. It has been found by Ammons (2) and Kimble and Horenstein (12) that the rate of dissipation of reactive inhibition is a negatively accelerated function of the length of interpolated rest.

C. *The Maturation Hypothesis of Dore and Hilgard.* Dore and Hilgard (4), utilizing one-minute trials and three different rest intervals (1, 3, and 11 minutes) with rotary pursuit learning found that the eleven-minute rest was the best, and that the three-minute rest was better than the one-minute rest. Dore and Hilgard attributed the superiority of the greater spacing to growth (maturation) which occurs *between* the practice periods, and which is related to the amount of stimulation provided *during* the practice periods. Thus, according to this view, the obtained scores are a "function of time rather than amount of practice (granted the necessary minimum)" (4, p. 258).

However, according to Dore and Hilgard, the maturation hypothesis ". . . can be shown to be necessary only if well-known processes describing increments and decrements between trials are found to be quantitatively insufficient to

account for the changes which occur" (4, p. 259). Actually, their results are completely understandable in terms of the present theory. The superiority of the greater spacing is produced not by growth during rest, but by the dissipation of reactive inhibition during rest. The score is not a function of time alone, but is related to the amount of reinforcement, the amount of work involved, and the length of the interpolated rest.

D. *The Learning and Work Factor Theory of Dore and Hilgard.* In an attempt to test Snoddy's theory, Dore and Hilgard (5) performed an experiment using the pursuit rotor which forms the basis of this additional theory. They hypothesized that if Snoddy is correct, with two equated groups having an equal number of trials distributed differently over the same total time, the group having decreasing lengths of interpolated rest (initial spacing and later relative massing) should be superior to the group having increasing lengths of rest (initial massing and later spacing). That is to say, initial spacing should be superior to initial massing if Snoddy's theory is correct.

However, Dore and Hilgard found the exact opposite to be true in their experiment, and as a result concluded that Snoddy's theory was inadequate. To account for their results, Dore and Hilgard formulated the learning and work factor theory according to which the learning factors produce improvement within practice (acquisition) and loss with non-practice (forgetting), while the work factors produce loss within practice (work decrement) and improvement with non-practice (recovery with rest).

It appears that this theory really does not conflict seriously with the present inhibition theory, and may be looked upon as an early and incomplete formulation of the latter. Instead of the learning factor concept Hull's system utilizes such constructs as habit strength and reaction potential, and instead of the work factor concept Hull's theory utilizes the concept of inhibitory potential. Also, the learning factor concept of Dore and Hilgard may imply a disuse theory of forgetting (loss with non-practice). If this is so, it represents a criticism to which the present inhibition theory is not subject.

E. *Bell's Theory of Interference and Warming Up Effect.* Another study specifically designed to test Snoddy's mental growth hypothesis is that of Bell (3). Utilizing the pursuit rotor, Bell introduced rests of one minute, ten minutes, six hours, twenty-four hours, or thirty hours early in learning (after trial 5) and late in learning (after trial 15). He found the optimum rest interval to be ten minutes, and further, that the one-minute rest was inferior to the much longer ones (6, 24, and 30 hours) when introduced early in learning, but superior to them when introduced late in learning.

Concluding that Snoddy's theory is inadequate, Bell formulated his theory according to which interference and warming-up effects operate *throughout* the learning process, but with differential effect at different stages. By interference is meant a kind of inhibition or conflict between incorrect and correct responses, this conflict often being observed as an obvious strain and lack of relaxation. Interference is greatest at the beginning of learning and gradually diminishes as learning progresses. On the other hand, during the first few trials, the warming-up effect increases rapidly and then remains fairly constant throughout the remainder of learning. The effect of rest is to remove interference, but to necessitate warming-up. Large gains are produced by a rest early in learning because the amount of interference removed is greater than the warming-up effect. However, late in learning there is a loss after rest because

the warming-up effect becomes greater than the amount of interference removed.

Again, these empirical findings appear to be adequately understandable in terms of the present inhibition theory which has room for Bell's warming-up effect, but which eliminates several of the difficulties inherent in his theory. The superiority of the ten-minute rest is understandable in terms of the negatively accelerated relationship between the dissipation of reactive inhibition and the length of interpolated rest. The finding that a one-minute rest becomes superior to much longer intervals when introduced late in learning coincides with the results of Humphreys' and Travis' experiments which were cited above. Late in learning, when there is relatively less reactive inhibition available to be dissipated during a formal rest interval (because of more resting *during* learning and therefore, more dissipation of reactive inhibition *during* learning), the introduction of a long interval of time will be less beneficial than early in learning because of the greater influence of such factors as the warming-up effect and retroactive inhibition. This is essentially substantiated by the findings of Hilgard and Smith (6). Using the pursuit rotor, they found a longer rest (3 minutes) to be superior early in learning and a shorter rest (1 minute) to be superior late in learning, while an extremely short rest (20 seconds) was always inferior to the one-minute rest. Also, they found decrements *between* daily sessions (24 hour interval) to occur only in the later stages of learning.

Finally, it should be noted that Bell's concept of interference seems to assume a differential forgetting theory of forgetting. That is, it seems necessary to explain the removal of interference during rest in terms of the more rapid forgetting of the incorrect responses. *If* the forgetting is attributed to disuse over the interval of time (this point is not specifically dealt with by Bell), Bell's theory is subject to the usual criticisms leveled against a disuse theory.

SUMMARY

This paper represents an attempt to reconcile the apparently divergent results of several studies, each concerned with the problems common to the motor learning situation, but each oriented towards a specific theoretical approach to these problems. It has been shown that not only are these studies explainable in terms of an extension of Hull's Performance Inhibition Theory, but that this approach leads to a consistency and unity in theory for which it is worthwhile to strive. The present theory is not being advanced as the last word, but rather as suggestive in nature and a stimulus for further research in the interest of a more generalized learning theory.

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OBJECTIVITY IN SYSTEMATIC AND "IDIODYNAMIC" PSYCHOLOGY

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I

In a recent paper Rosenzweig (7) elucidated certain methodological aspects of "idiodynamic" psychology and projective techniques which he strongly suggested are "as much a threat to the traditional psychologist as is the postulate of response dominance. Both signalize the eclipse of scientific 'objectivity' " (7, p. 216). This threat is presumed to be a consequence of what he calls three "idiodynamic personality postulates" (7, p. 214). It would appear that, like Koch (3), Rosenzweig conceives of postulates as implicit definitions, and that his three "postulates" are statements indicating some defining properties of projective techniques and "idiodynamic" psychology. It seems clear, therefore, that these "postulates" are elevated to high theoretical status.

It is our purpose in this paper to analyze and clarify the theoretical status of these "postulates." Although we shall deal mainly with Rosenzweig's recent paper, we wish to emphasize that to the extent that the notions therein advanced enjoy quite wide acceptance in many clinical areas, our discussion has a fairly general significance.

In the history of psychology it has sometimes occurred that an event or a set of events of significance only in the biography of the science has been elevated to the status of "theory"; and this circumstance has led to confusion which has sometimes required years of critical exchange to clarify. Two apt illustrative instances come to mind: Early behaviorism consisted of propositions asserting (1) the elimination of "mind" and "consciousness," (2) the universal validity of the law of exercise as *the* law of learning, and (3) the banishment of heredity. Had it been customary then, as it is now, to call such statements "postulates," these would no doubt have been termed "three postulates of behaviorism." The mischief of such a state of affairs lies in the fact that these do not constitute a logically coherent set of propositions, but a cluster of opinions which *happen* to have had a certain locus. Since the cluster *qua* theory is accidentally defined (i.e., since these propositions do not jointly entail the theorem system) it is easy to see that no inconsiderable confusion might result from raising them to the status of a theory.

A second illustration, fortunately, is made available by a very recent paper presented by Meehl and MacCorquodale (5). These writers, in a penetrating analysis of the nature of expectancy (cognitive) learning theory, have shown that a number of properties which have been commonly held to be *definitive* of such expectancy theory (e.g., Gestalt-configural stress, discontinuity view of discrimination learning) are, in fact, such accidental associations as we have described. It is our contention that such a state of affairs has come about in the literature of "idiodynamic" personality theory and projective techniques, and that the "postulates" cannot, in fact, present the supposed threat to systematic and experimental psychology.

We may begin with a statement of neutrality with respect to the acceptability and respectability of the unique personality (i.e., the individual) as a proper

subject for scientific investigation. It would appear that the credentials presented by many of the ingenious workers in the field are *bona fide*. These credentials, in the form of highly adequate investigations, have won recognition for the legitimacy of this kind of work in psychology. However, in its enthusiasm the new psychology is sometimes led to claim revolutionary innovations which (as we shall see) should be credited to others.

II

Rosenzweig's three "idiodynamic personality postulates" which "the projective techniques embody" are "response dominance, configuration dominance, and idioverse dominance" (7, p. 214). What very considerable significance these are thought to hold for "traditional" psychology may be strikingly illustrated by the following two sentences: "To the traditional psychologist this new state of affairs presented a decided threat to the objective nature of science" (7, p. 215). "Stated thus baldly the postulate of configuration dominance is as much a threat to the traditional psychologist as is the postulate of response dominance" (7, p. 216).

We should like to consider these "postulates" in reverse order. In assigning "idioverse dominance" the status of a "postulate" (i.e., an implicit definition) of projective techniques he has elevated an accidental set of circumstances to the dignity of a theoretical formulation. It is, of course, *historically* accurate to note that the same workers in psychology who were interested in developing "idiodynamic" psychology were also interested in the projective techniques. But the *definition* of the idioverse is *any* set of scores, measures, or events with respect to a *population of occasions*. There is, of course, no theoretical, no logical reason why this population may not be scores of structured as well as unstructured instruments. Indeed, Cattell (1), whose ipsative laws are precisely such idioverse laws, argues strongly *against* the use of projective techniques. Further, we should like to call attention to the admirable clarity with which Cattell has described, in his covariation chart, the kinds of covariation which may be studied in the idioverse.

If we formalize the argument presented by Rosenzweig, we find it something like this:

Proposition A: "Idiodynamic" psychologists are interested in the idioverse, i.e., the population of occasions for a single individual.

Proposition B: "Idiodynamic" psychologists have consistently used projective instruments to sample occasions for a single individual.

Conclusion: The use of projective techniques for the systematic sampling of occasions for a single individual is a defining property of "idiodynamics."

Such an argument is formally invalid. Far from being a "postulate" which is "embodied" in projective techniques, this is a historical accident which is *theoretically irrelevant* for the definition of the idioverse.

Considering next the "postulate" of "configuration dominance," we find ourselves at some disadvantage in that we are not entirely clear as to the intent of the argument. We present, however, several considerations without claiming that they exhaust the possible interpretations of the configuration argument.

(a) *Configuration on the Stimulus Side.* The classical Gestalt psychologists (Wertheimer, Koffka, Köhler) have demonstrated experimentally that configural specifications are required for the stimulus field in order to predict the response side. Thus if an animal is trained in a discrimination problem with a single light it is not possible to predict

by "summation" what the behavior will be in the presence of three lights. Moreover, "three-ness" is in itself not an adequate specification of the stimulus field; in addition there is required "verticality," "horizontality," "triangularity," etc. Now again we are confronted with the interesting *historical* fact that workers with the projective techniques have been interested in Gestalt psychology. They have quite accurately called attention to the significance of recognizing the configural properties of the stimulus field and their stimulus material. *But this does not prove to be a defining property of the projective techniques.* Indeed, the configural significance of *structured* instruments can easily be illustrated. Consider a fictitious structured personality test composed of three hypothetical true-false items:

1. Most notags karolize elatically.
2. I do not think lapzigs and patniks should be admitted.
3. I would not trust alyps under any circumstances.

Assume, further, that it has been experimentally established that items 2 and 3 have distinct personological significance but that item 1 has been shown to be "neutral" in that it cannot be found to differentiate any criterion groups. *It cannot be deduced that this item can be removed from the inventory without consequences which would materially affect the meaning and validity of the inventory as a whole.* Indeed, the makers of structured inventories have been known to retain "dead" items because their removal influences response to other items, that is to say, the configural aspects of the stimulus field are altered. The conclusion is inescapable that the configural significance of the projective techniques on the stimulus side is not a *defining property.*

(b) *Configuration on the Response Side.* It is a fact worthy of note that, although the notion of configuration (or pattern) is invoked with great frequency with respect to the productions of personality (i.e., on the response side), it has not been considered necessary to formalize (i.e., define) the notion, at least beyond such incidental comments as "identical quantitative values for any one of the Rorschach factors may have different psychological meanings depending upon the total pattern of scores in which the quantity is imbedded" (7, p. 221). But if this simple notion (i.e., that given any two scores their *joint* consideration will more accurately predict a criterion than will either score alone)—if this notion *defines* the configural notion on the response side, then we can only point out that while it is *historically* true that Rorschach workers have been diligent in exploring this property of their instruments, this does not entail the notion that it is "embodied" in the techniques in the sense of *differentially defining* them. Indeed, this concept has been presented for structured instruments with some mathematical elegance by Meehl (4), who has shown how it is theoretically possible for either of two items to correlate zero with a criterion while both items together (i.e., scored configurally) may differentiate *perfectly!* That is to say, although neither a normal nor a schizophrenic group may answer any *single* item in a consistent direction (50 per cent of each group answering each item "true" and 50 per cent "false"), the normals answer the *two* items in the *same* direction and the schizophrenics, in *opposite* directions (4, p. 166). It therefore appears inescapable to us that the "postulate" of configuration dominance on the response side cannot be considered as a defining property of projective techniques. Considering scale scores rather than separate items, it is a simple matter to dem-

onstrate that for so structured an instrument as the Minnesota Multiphasic Personality Inventory "identical quantitative values" may have "different psychological meaning" (7, p. 221). Consider two MMPI profiles in which the *T* scores for the *D* (depression) scale are exactly alike (e.g., 70) and in which the *Pt* (psychasthenia) scores are likewise identical (e.g., 68). It would be a naive clinician, indeed, who would commit the error of assigning similar "psychodynamic meanings" to these scores without consideration of the rest of the profile.

(c) *Configuration as a "Whole-Part" Issue.* One not infrequently finds the statement that the "whole is more than the sum of its parts" (2, 6); and it has even been suggested that this constitutes one of the "basic issues on which the major theories [of learning] divide" (2, p. 9). If it is *this* notion which is presumed to be embodied in the "postulate of configuration dominance" then we would venture the suggestion that this is too vague a notion to be fruitful. In the first place, what *kind* of statement is this supposed to be? That is to say, is it offered as a *formal* truth or does it purport to incorporate *empirical* content? If formal, it is a palpable contradiction without redefinition; as long as "whole," "part," "more," and "sum" mean what they were initially intended to mean by definition, the "whole-is-more" doctrine is meaningless. If the proposition, however, is supposed to have empirical content, to say something about the nature of behavior, we require, in order to understand it, operational specification. If, in the last analysis, what Rorschach workers mean by this postulate is that any two scores considered jointly (i.e., configurally) will more accurately predict a criterion than either score alone, then they appear actually to be redefining the notions of "whole" and "sum."

Of the "postulate" of response dominance we may ask to what extent it constitutes "a new state of affairs" which "represented a decided threat" (7, p. 215) to the objectivity of the traditional psychologist. In view of the revolutionary significance which is attributed by "idiodynamic" psychologists to the "dethroning of the stimulus" (7, p. 215), we call attention to the following passage:

> "With the discovery of the stimulus and the collection of a large number of specific relationships of stimulus and response, it came to be assumed by many writers that all behavior would be accounted for in this way as soon as the appropriate stimuli could be identified. Many elaborate attempts have been made to establish the plausibility of this assumption, but they have not, I believe, proved convincing. There is a large body of behavior that does not seem to be *elicited*. . . . This kind of behavior might be said to be *emitted* . . ." (8, pp. 19–20).

This is taken, as most readers will recognize, not from some psychodynamic psychologist, but from Skinner's *Behavior of Organisms.* Furthermore, the distinction between a *discriminative* and an *eliciting* stimulus which Rosenzweig thinks "may now be clarified" (7, p. 215) has also been described at some length by Skinner (8, p. 236). And in his summarizing description of Skinner's system Hilgard has, of course, called attention to the fact that some of the relationships are "very different in the case of the discriminated operant" (2, p. 117) from those of the respondent. It is hardly necessary, we hope, to emphasize that we do not call attention to the fact that the operant was well established in psychology before either psychodynamics or the projective techniques merely to rectify an interesting chronological error. There is a more fundamental methodological point, namely, that the "dominance of the response"

entails neither the abrogation of the conventional experimental criteria nor the twilight of traditional objectivity. This should be obvious from the fact that such a "dethroning" of the stimulus as Rozenzweig describes may be credited to a psychologist who may in some sense be designated an arch-behaviorist.

In closing this discussion we should like to point out that we have restricted our remarks to what we believe to be the more clearly drawn issues which have been raised, and have left for separate treatment the status of such problems as are posed by phenomenological psychology and its relation to "idiodynamics."

SUMMARY

We have tried to show that the "postulates" which have been said to be "embodied" in the projective techniques (i.e., response dominance, configuration dominance, and idioverse dominance) are also properties of non-projective (i.e., structured) instruments, and that the association of these properties with the projective techniques exclusively is a matter of historical accident which is, for *theoretical* purposes, irrelevant. Indeed, only one of the "postulates" (that of idioverse dominance) may be said to define the task of the "idiodynamic" psychologist; which task is essentially the discovery of laws of occasions (Cattell's ipsative laws) for a single individual. We have shown that with respect to the "postulate" of response dominance, the "revolution in the conception of the stimulus" was instigated by Skinner rather than by psychodynamic or "idiodynamic" psychologists.

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THE FORMATION OF NEGATIVE HABITS: A NEUROPHYSIOLOGICAL VIEW [1]

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Preliminary Remarks on Inhibition

Whenever the magnitude of an organism's response to a given stimulation falls short of a theoretical maximum it is evident that factors are in play that depress the response. Such depression of activity is called *inhibition*. It may come about in a variety of ways. It may be due to gross physical factors, such as injury or disease; or to more subtle and transient events, like fatigue, diminished conditions of need, or other physiological variations. Again, the inhibition may be attributable to the fact that the organism happens at the time to be reacting to some other stimulus which evokes a response incompatible with the given response. The pattern of this kind of inhibition is to be found in the *reciprocal inhibition* of one spinal reflex by another (28, pp. 83–107), such as occurs, for example, when stimulation of the ipsilateral afferent nerve causes relaxation of a vastocrureus muscle contracting to a contralateral stimulus. Finally, a response may be diminished in strength following the *learning* of a negative reaction tendency, i.e., the learning of inhibition of the response. This is called *conditioned inhibition*.

Diminution of responsiveness due to conditioned inhibition has the kind of permanence associated with learning in general. But its development depends upon the occurrence in the organism, in contiguity with the relevant stimulus conditions, of individual inhibitions of response of a more transient kind—fatigue or reciprocal inhibition. We shall consider how conditioned inhibition develops on the basis of each of these two types of transient inhibition.

Extinction and the Conditioned Inhibition Associated with Fatigue

The term extinction is applied to the more or less smoothly progressive weakening of the response to a conditioned stimulus when it occurs a number of times without reinforcement. A partial recovery of the strength of the response is noted if the stimulus is not applied for some time, and this is what is known as spontaneous recovery. The fact that spontaneous recovery is only partial leads inevitably to the inference that in the inhibition of response associated with extinction two distinct elements are involved—an inhibitory state that dissipates with time (reactive inhibition), and an enduring diminution of response due to a negative conditioning having occurred. The recognition of this dichotomy has led to the first reasonably convincing explanation of the extinctive process, in the form of what Hull has called the Mowrer-Miller hypothesis. The most thoroughgoing and elaborate exposition of this hypothesis to date is due to Hull himself (10, pp. 277–302), although the basic propositions were originally put forward by Mowrer and Jones (20) and by Miller and Dollard (19).

The initial supposition of the Mowrer-Miller hypothesis is that each time an organism makes a response to a stimu-

[1] The experiments referred to in this article were performed in the Department of Pharmacology, University of the Witwatersrand. They were subsidized by a grant from the South African Council for Scientific and Industrial Research.

lus, whether the response is reinforced or not, a fatigue-associated state or substance is brought into being that has an inhibitory effect upon a closely following evocation of the same response. This fatigue-associated state is regarded as the basis of reactive inhibition, and insofar as fatigue implies a disturbance of the equilibrium of the organism, a state of drive is postulated as a direct function thereof. This drive will normally be reduced when the activity that produced it ceases. *Cessation* of the activity will be in closest contiguity with the drive reduction, and will consequently become conditioned to any afferent impulses or stimulus traces which chance to be present at the time. The stimuli conditioned to the response that caused the fatigue will be responsible for some such traces. These stimuli will therefore now in some measure be *conditioned to an inhibition* of the response to which previously they were positively joined. As a result, the strength of that response to the next presentation of these stimuli will be found to have decreased to some extent, even though an interval has been allowed to lapse long enough to permit the dissipation of all reactive inhibition. Of course, no decrement of response is, as a rule, noted when the response is a reinforced one; for in that case positive learning effects may overshadow the effects of conditioned inhibition.

The Mechanism of Reactive Inhibition

There is evidence that reactive inhibition is associated with fatigue of muscle. In the first place, it has been found that reactive inhibition is cumulative in circumstances in which fatigue is cumulative. It is well known that fatigue mounts when responses are massed, and this is also the case with reactive inhibition. When reinforced responses are sufficiently massed a *decreasing* strength of response may be noted. This phenomenon was first described by Pavlov (23, p. 247). Like fatigue, the decremental influence disappears with time (7, p. 148). In experiments in which extinction is commenced soon after massed learning trials, the presence of reactive inhibition seems to be revealed by an initial rise in the amplitude of the response (8, 9, 29). Accumulative reactive inhibition is also produced during extinction, as shown by the fact that the rate of extinction is increased if extinction trials are massed (18, 24, 25, 26). There is also evidence of a correlation between amount of fatigue per response evoked and degree of reactive inhibition. The greater the effort involved in a response the sooner is extinction accomplished (4, 11, 20).

But however close the relationship between fatigue and reactive inhibition may be, they are not merely different labels for the same thing. Even though the fatigue-associated substance that underlies reactive inhibition is presumed to be produced in muscle, reactive inhibition *cannot be due to a local action of this substance,* because the same response that disappears after repeated non-reinforced presentations of a conditioned stimulus may at once reappear if another stimulus to which also it has been reinforced is presented. Pavlov (23, p. 158) showed this in an experiment in which a dog was conditioned to make more or less equal salivary responses to tactile stimuli at any point on its skin. One minute after extinction of the response to stimulation of the left shoulder, stimulation of the left thigh evoked a practically undiminished response. This is in line with some observations of Sherrington on spinal reflexes (28, p. 219). He found that when the scratch reflex elicited by

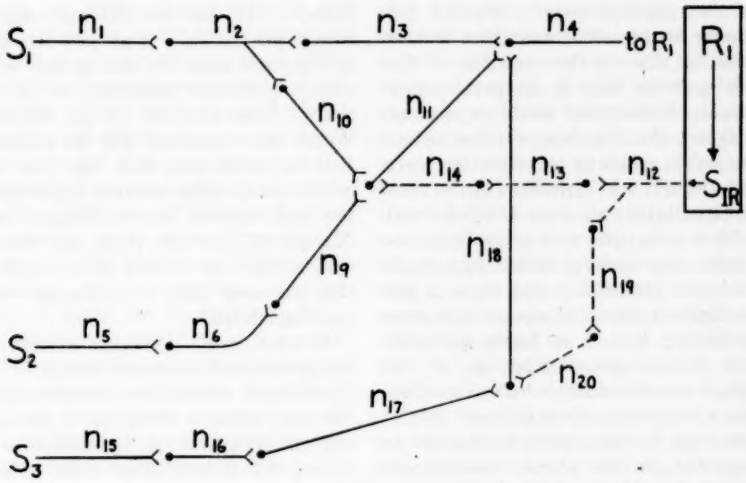
stimulation of a point on the skin becomes weakened after many repetitions, it recurs with renewed vigor on stimulating a point two or more centimeters from the original one.

It seems, then, that *neural* mechanisms must play a part in the production of decrement due to repetition in both the spinal reflex and the conditioned response. If a substance produced by activity of muscle leads to the decrement, this substance presumably acts by stimulating nerves whose excitation creates an inhibitory effect at certain of the synapses transmitting impulses producing the muscular activity. A direct demonstration that this is indeed the mechanism of reactive inhibition is lacking, but there are facts that give important indirect support to the supposition. First, Gellhorn and Thompson (5) have shown that muscular activity produces some substance that inhibits reflex responsiveness. They found, in ten subjects selected for their very active triceps reflexes, that making the arm ischaemic did not diminish the reflex. But when, in addition, the muscle was repeatedly exercised until it became painful, the reflex could not be evoked in any of the subjects, even by strong stimulation. On restoring the circulation, the pain disappeared at once, and the reflex returned in 1½ to 3 minutes. Secondly, there is evidence that the afferent nerves of muscle carry fibres whose stimulation results in inhibition of that muscle's reflex responses. Sherrington (27) found that if the central end of the cut afferent nerve to a muscle whose motor supply is intact is stimulated, not only is there no reflex contraction, "but the muscle, if reflexly contracting (to another stimulus) is thrown out of contraction." *A possibly decisive experiment would be to determine the effect on extinction of depriving the chief muscles subserving a response of their afferent innervation.*

A Neurophysiological Hypothesis

Meanwhile, going with the weight of evidence, and assuming the correctness of the above-mentioned view of the mechanism of reactive inhibition, it seems reasonable to attempt to show, in conformity with principles previously expounded (33, 36), how conditioned inhibition *could* be subserved by synaptic processes. In the same way as positive learning seems to depend upon certain neural chains becoming increasingly conductive, it may be expected that conditioned inhibition implies decreased conductivity of these neural chains. We may see by reference to the figure how this may come about, always remembering that each neurone in the figure represents a very large number in the organism. The sequence of neurones n_1 to n_4 represents the linkage between an electrical stimulus applied to a foot (S_1), and the final common path (n_4) to a foot-lifting response (R_1). Due to a functional connection having been established between n_0 and n_{11}, as described previously (36), another stimulus energy, S_2, now evokes the response R_1 by means of impulses traversing the neural sequence n_5–n_0–n_9–n_{11}–n_4. The fatigue-associated substance (S_{IR}) which is produced in the muscles that carry out R_1 stimulates the endings of nerves whose central connections are inhibitory at certain synapses on all pathways to R_1. The inhibitory chain, insofar as it affects the n_0–n_{11} synaptic zone, is represented by n_{12}–n_{13}–n_{14}.

Inhibitory impulses, once set up, must be assumed to arrive indiscriminately at synapses on all pathways leading to the final common path to R_1 (n_4). But since other pathways are evidently less affected when S_2 is extinguished than is the pathway subserving S_2 itself, it must be assumed that the inhibitory effect occurs most powerfully at synapses at



KEY

Pathways of excitation of R_1.

Pathways of inhibition of R_1.

S_{IR}—Stimulus to reactive inhibition, produced by R_1.

Diagram illustrating the development of conditioned inhibition based on reactive inhibition.

which recent transmission of impulses has produced certain sensitizing preconditions. Thus, in the figure, let S_3 be another stimulus energy also conditioned to the response R_1, and leading to R_1 by excitation of the sequence n_{15}–n_{16}–n_{17}–n_{18}–n_4. When S_2 has evoked R_1 inhibitory impulses arrive at the synapse $n_{17} \cdot n_{18}$ from the sequence n_{12}–n_{19}–n_{20}, but the effect of these impulses on $n_{17} \cdot n_{18}$ will be relatively slight if no impulses have recently been passing from n_{17} to n_{18}. Synapses whose activity has been instrumental in bringing about an effector response, become, through this very activity, more sensitive to the inhibitory impulses that are consequent on the response.

With this in view, it is easy to see that *conditioned inhibition* can be understood in neurophysiological terms as a process that to a great extent parallels positive learning. It may easily be imagined that the *cessation* of impulses from the inhibitory neurone n_{14} acts on the synaptic zone $n_9 \cdot n_{11}$, sensitized by its previous activity, in such a way that conductivity between n_9 and n_{11} is diminished.

The above neurophysiological hypothesis has *an important immediate consequence.* It rescues the Mowrer-Miller hypothesis from the following devastating criticism. Suppose that a rat has learnt to press a pedal as a result of previous food rewards. On any occasion when no food follows there will be, according to the Mowrer-Miller hypothesis, a weakening of this pedal-pressing habit, because cessation of the movement is closer in time than performance of the movement to the reduction of the fatigue-associated drive. But what happens when food does fol-

low the pedal-pressing? Clearly, the ensuing hunger drive reduction is also closer in time to the cessation of the pedal-pressing than to the performance thereof. Consistency would require one to expect that the hunger drive reduction would reinforce the cessation more than the act! As experience belies such an expectation, we must conclude that if drive reductions were altogether nonspecific the Mowrer-Miller hypothesis would be untenable. But there is not the slightest reason to assume such nonspecificity; and it is highly probable that special neural relations of the fatigue-associated drive make its reduction a reinforcer of conditioned inhibition only, by some such mechanism as suggested in the above neurophysiological hypothesis, while other drive reductions only reinforce *positively* the activities close to them in time.

It may be noted that our hypothesis dispenses with the Mowrer-Miller idea that in the conditioning of inhibition a *cessation* of an act is reinforced in the same sense as the act itself would be reinforced. Cessation of an act is relevant only insofar as it makes possible reduction of the fatigue-associated drive, for this is what determines *conditioned* inhibition—the "permanent" lessening of conductivity of the synapses subserving the evocation of the act in response to the particular stimulus constellation.

THE DEVELOPMENT OF CONDITIONED INHIBITION BASED ON RECIPROCAL INHIBITION

It has long been realized that habits are often eliminated by enabling new habits to develop in the same general situation (7, pp. 108–114), but hypotheses as to the mechanisms by which this occurs have been notably vague. That reciprocal inhibition (v.s.) is concerned in the weakening of old responses by new ones was realized by Wendt (31), but he did not suggest *how* a permanently weakened strength of reaction came to follow the temporary reciprocal inhibition of an activity. Like Guthrie (6, pp. 64–84), Wendt was concerned with an explanation for extinction, and, like him, regarded competition between reactions as the basis for it; but, as Hilgard and Marquis (7, p. 118) point out, this is contradicted by several facts, such as the increased rate of extinction with massing of trials.

If conditioned inhibition is built up during extinction because traces of the conditioned stimuli are contemporaneous with reactive inhibition of the conditioned response, it is reasonable to expect that if some other cause were to inhibit the conditioned response, conditioned inhibition would likewise be built up, being reinforced by whatever drive reductions happened to be contiguous.

One way in which the conditioned response could be inhibited would be by the simultaneous presentation of a stimulus leading to another response which is incompatible with the conditioned response, and which commands a stronger reaction potential than the latter. In such circumstances the conditioned response will be reciprocally inhibited. If a major drive-reduction is present at the time, a significant amount of conditioned inhibition of the response will be developed. This process is illustrated by a striking experiment of Pavlov's (23, pp. 29–31), in which, however, the response to which conditioned inhibition was established was actually an "unconditioned" response. A weak electric current was made a conditioned stimulus for food in a dog. The current was in time gradually increased (with feeding) until it was extremely strong, but even then no defense reaction was manifested. In other words, the pathways normally con-

necting the electrical stimulus with the defense reaction had become inhibited. It would appear that at every stage of the experiment the performance of the feeding response involved a reciprocal inhibition of the mild defense reaction aroused by the electrical stimulus. Because of the drive-reduction consequent on the feeding, this inhibition of response became progressively more strongly reinforced to the electrical stimulus. After many repetitions of the procedure, in the course of which the current was gradually stepped up, so great a degree of conditioned inhibition of the defense reaction to the current was established that even very strong electrical stimuli were unable to evoke that reaction, but evoked only the feeding response.

An Experimental Test of the Nature of the Process by which Reciprocal Inhibition Weakens Habits

The question that arises regarding such an experiment as the above is whether an inhibitory neural change has been produced, or whether there is merely a suppression of response that depends upon the new response continuing to dominate the original one. In the course of an extensive series of experiments on animal neuroses reported elsewhere (32, 34, 37), the present writer has made some observations that appear to give a clear answer in favor of a "permanent" inhibitory neural change. The relevant facts will be briefly given below.

Cats were made neurotic by administering to them high voltage low amperage shocks while they were confined in a cage 40″ long, 20″ wide, and 20″ high. The procedure resembled in general that described by Dimmick, Ludlow, and Whiteman (3) and by Masserman (17); but it may be noted that whereas these authors thought that conflict was essential to the production of the neuroses because before shocking they trained *all* their animals to conditioned feeding reactions in the experimental cage, the present writer obtained much the same results whether the shocked animals had had this preliminary food training or not. Just before a shock an auditory stimulus, such as a buzzer, was invariably sounded. After an animal had received several shocks, it was found that if it was brought back to the experimental cage on a subsequent occasion, it showed reactions very much like those that had been observed at the time of the shocking, the keynote being "anxiety." The reactions included resistance to being put into the experimental cage, muscular tenseness, mydriasis, hypersensitivity to indifferent stimuli, and refusal to eat meat pellets even after having been starved during the previous 72 hours. Certain animals displayed such additional reactions as howling, crouching, pilo-erection, trembling, urination and tachypnoea. The reactions could always be intensified by presenting the auditory stimulus that had preceded the shocking, as might have been expected. The various reactions were manifested at high intensity even after months of freedom from experimentation, or if, alternatively, the animals were replaced in the experimental cage repeatedly without further shocking. The latter circumstance amounts to a marked resistance to extinction (which is a characteristic of neurotic reactions in both animals and man).

Thus, in order to eliminate these reactions special measures had to be considered.[2] As already stated, in the experimental cage the anxiety reactions were associated with inhibition of eating even after 72 hours' starvation. This suggested that under different conditions eating might inhibit anxiety reactions:

[2] The therapeutic implications of the measures employed have been discussed elsewhere (32, 34, 38).

in other words, that the two responses might be reciprocally inhibitory. Now, it had been noted that the inhibition of eating, with the rest of the anxiety picture, occurred not only in the experimental cage but also anywhere in the experimental room, and in other rooms resembling this room in various ways—an effect presumably due to primary stimulus generalization (35). The slighter the resemblances of a given room to the experimental room the less marked were the anxiety reactions. By trial and error, in the case of each animal a room was sooner or later found in which he would eat although displaying some evidence of anxiety. If the animal was repeatedly fed in this room, it was found that he ate there with increasing readiness and quite soon ate in another room in which he would not have eaten before. It was thus possible to work back from room to room until the animal would eat in the experimental room and at last in the experimental cage, eventually showing no trace of anxiety there.

This means that the numerous visual and olfactory cues that had been conditioned to the anxiety reactions no longer evoked them. But if the *auditory stimulus* that had preceded the shocks was now presented the animals again manifested a high degree of anxiety. The effect was such that the animal could be inhibited by the auditory stimulus from completing any movement towards a pellet of meat in the experimental cage, and, in most cases, at various points in the experimental room as well. (If this was done repeatedly visual cues again inhibited eating.) The problem was to present the auditory stimulus in such a way that the anxiety reactions would be weak and there would be no inhibition of eating. Three solutions were found —diminishing the physical intensity of the stimulus, increasing the distance between the animal and the stimulus, and making use of the stimulus trace and the fact that the effects of a brief sensory stimulus on the nervous system gradually decline in intensity with passage of time (1). The last method was applied to seven animals and will be briefly described.

The cat was placed inside the experimental cage, and though showing no sign of disturbance, was given one or two meat pellets on the floor of the cage. Then, after an interval of about a minute, the auditory stimulus was sounded for about one-fifth of a second, and *immediately afterwards* a pellet was dropped in front of the animal, which had in the meantime become tense and hunched-up, with pilo-erection and mydriasis. After a variable interval, usually about 30 seconds, the animal would have lost most of its tenseness and would move forward cautiously to the pellet and eat it. Not less than one minute later, the auditory stimulus would again be presented and be followed by a meat pellet, again making the cat tense, but this time the animal would eat the pellet after a shorter interval. This procedure was repeated 10 to 20 times during a session, and the interval until the eating of the pellet would gradually diminish until the animal ate without delay. In illustration of the above, the intervals recorded (in seconds) for one cat during her first session of this kind were 40, 15, 20, 7, 5, 6, 6, 6, 4, 8, 8, 11, 4, 6, 4, 4, 5, 3, 4. At the next session the intervals were 3, 3, 2, 3, 2, and 2 seconds for the first six presentations of the stimulus, and thereafter she consistently responded practically at once.

The next step was *to increase the duration of the auditory stimulus*. First, every fourth or fifth stimulus was given a duration of about a second; and

then, gradually, depending on the responses of the animal, the duration was increased. Eventually the animal would show no vestige of anxiety even to a stimulus of 30 seconds' duration, and would make alert food-seeking movements as soon as it heard the auditory stimulus.

The final step in the procedure was *to extinguish the feeding response.* Each animal was given 30 irregularly massed extinction trials on each of three successive days. Long before the end of the third day's session they all showed almost complete indifference to the auditory stimulus. Immediately after the conclusion of the third extinction session the following test was made. A pellet was dropped on the floor of the experimental cage about 2 feet away from the animal, and as he began to approach, the auditory signal was sounded continuously to see if extinction had reinstated the inhibitory effect on eating that had originally been noted. *In no instance was there observed any semblance of the restoration of an anxiety response or any suggestion of an inhibition of eating.* Moreover, observation for many weeks afterwards never revealed recurrence of anxiety responses in any animal.

These results are reasonably accounted for as follows. When stimuli to incompatible responses are present simultaneously the occurrence of the response that is dominant in the circumstances involves the reciprocal inhibition of the other. The feeding response was able to dominate and reciprocally inhibit the anxiety responses in the circumstances provided by the above experiments. Gradually, a "permanent" change developed, so that to stimuli to which there was initially a response of the anxiety pattern there was finally a feeding response with inhibition of anxiety. This implies a new positive conditioning *pari passu* with a conditioning of inhibition, and this could have been subserved by at least two drive reductions—hunger drive reduction, and reduction by the reciprocal inhibition itself of the drive (central neural excitation [33]) antecedent to the anxiety responses. That the conditioned inhibition was due to *a neural change* and was not merely a correlate of the occurrence of the alternative response seems a clear inference from the fact that eventual extinction of the alternative response did not result in reinstatement of the original one. The mechanism of this neural change may be imagined to be analogous to that proposed above for the case of extinction.

Retroactive Inhibition as a Case of Conditioned Inhibition Based on Reciprocal Inhibition

By retroactive inhibition is meant the decrement in retention of learned materials resulting from activities interpolated between an earlier and a later test of retention. It is thought that interpolated activities are the cause of ordinary forgetting, reducing the strength of the connection between a cue and a learned response (14, pp. 453–455). A very potent consideration in support of this idea is the observation reported by Jenkins and Dallenbach (12) and later confirmed by van Ormer (30) that recall of a learned list of nonsense syllables is practically undiminished after an interval of from 1 to 8 hours spent in sleep, but decreases progressively with every hour if the subject is awake.

Osgood (21, 22) has suggested a hypothesis explaining retroactive inhibition in terms of increasing inhibitory habit tendencies based on reciprocal inhibition. In general agreement with him the following formulation is pro-

posed. If, after the learning, another activity takes place which involves the same or similar cues but a dissimilar response, there will be at the time a reciprocal inhibition of the response originally learned. Because of the contiguous drive reduction—which is assumed to be present because every stimulation creates drive and therefore the potentiality of drive reduction (33)—this reciprocal inhibition will leave in its wake a conditioned inhibition, a "permanent" weakening of the connection between the originally learned cue and its response. It would be expected, if this hypothesis is correct, that the amount of impairment of recall will be the greater the more closely the cue to an interpolated activity resembles the cue to the learned response; and given similar cues, the impairment will be the greater the more the responses differ.

There is considerable evidence in support of these expectations. McGeoch, McKinney and Peters (16) found that if an item *A* is the learned cue to a response *B*, and the association *A–C* is then learned, there is much poorer retention of the original association than if the association *D–E* is interpolated. A decrement also occurs with the interpolation of *D–E*, but as McGeoch points out (14, p. 492), there are aspects of *D* which are similar to *A*. Bunch and Winston (2) showed that if, after *A–B* has been learned, *C–B* is interpolated, there is much less interference with the *A–B* relationship than there is when *A–C* is interpolated. It has been shown by McGeoch and McDonald (15) and by Johnson (13) that retroactive inhibition is the greater the more closely the learned and the interpolated *cues resemble* each other in meaning; and Osgood (21, 22) has found that if the learned and interpolated cues are identical, there is more interference the more the *responses differ* in meaning.

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THREE ESTIMATES OF DIFFERENTIAL EXCITATORY TENDENCY[1]

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The analysis to be presented in the present paper is based upon data collected by the author (2) in an experiment[4] designed primarily to study the effect on instrumental behavior of changes in the time of delay of reinforcement. Rats were trained to respond to two bars, reinforcement being delayed one second at one bar and five seconds at the other. The animals developed a significant preference for the short delay bar, as determined on the one-fourth of the trials during which both bars were presented simultaneously to the subject. On the remaining trials only one bar was presented, thus permitting the observation of the speed of response to each bar separately. It was found that the response speed to the short delay bar became significantly greater than that to the long delay bar. When the delays of reinforcement were reversed (that is, when the one-second bar was changed to a five-second delay of reinforcement and vice versa) there was noted a gradually lowered response speed to the originally-short-now-long delay bar and a corresponding gradually increased response speed to the originally-long-now-short delay bar, both response speed curves adjusting to the appropriate levels after approximately 120 post-reversal trials to each bar. A comparable change in preference also followed the reversal in the delays of reinforcement, the per cent choice of the originally short delay bar falling from significantly above chance to significantly below chance.

The data therefore permit the plotting of three response measures from the same animals throughout the course of the experiment. These measures are: (1) response speed, (2) per cent choice of the short delay bar, and (3) preferential latency of the short delay bar. It is the purpose of the present article to define these measures operationally, to describe the assumptions made by Hull (1) in relating each measure to differential excitatory tendency, and to present the obtained relationship between these three estimates. It should be noted that, because the last two meas-

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[4] The apparatus used in this experiment was a double-bar Skinner-type box, into which the animals entered from a starting alley. A bar-touching response was required, one bar response being followed by reinforcement after one second and the other after five seconds. After 80 trials to each bar under the original delay conditions for the experimental group, and after 144 trials to each bar under the original delay conditions for the control group, the delays of reinforcement were reversed, the one second delay bar being changed to a five second delay of reinforcement, and vice versa. Data were collected in blocks of four trials each. The first trial was a free choice trial, with both bars presented to the subjects. The remaining three trials were forced to equate training to each of the bars; only one bar was presented on forced trials. Per cent choice of the short delay bar was obtained on the first trial of each block. A latency measure of response strength was obtained on the second and fourth trials, which were always forced to opposite bars. Details of the experimental design are presented elsewhere (2).

ures yield information concerning only the *difference* between two excitatory tendencies (that to each of the bars), this section will be concerned with three estimates of this differential excitatory tendency.

Response speed is the reciprocal of the obtained latency measure. Hull has defined latency ($_st_r$) as the time between the onset of the stimulus and the beginning of the instrumental response, and has assumed that this latency is some reciprocal function of the momentary effective excitatory tendency ($_s\dot{E}_R$). The reciprocal of latency (response speed) is therefore some linear function of $_s\dot{E}_R$. It should be noted that in the present experiment the latency obtained is not strictly as defined by Hull, but rather a response time or duration. By more molecular analysis, however, this response *time* could be considered as a summation of a number of response *latencies*, thus justifying the conventional use of the word latency in naming this response measure; indeed, data will be presented which substantiate the notion that response speed, as herein obtained, is linearly related to $_s\dot{E}_R$. Hull has further assumed that the standard deviation of oscillation is constant, except perhaps in the earliest portion of learning. It therefore follows that the *mean* $_s\dot{E}_R$ has a constant displacement from $_s\bar{E}_R$, being 2.5 sigmas of oscillation below this latter effective excitatory tendency. Since the standard deviation of oscillation is assur d to be the same under high and low performance levels (and therefore short and long delay conditions) for the same subjects, the difference between the mean $_s\dot{E}_R$'s will equal the difference between the two $_s\bar{E}_R$'s, both of the former means displaced 2.5 sigmas below the latter excitatory strengths. The obtained difference in mean response speed will therefore yield one linear measure of the differential excitatory tendency to the two bars.

A mathematical derivation of this difference measure (D_{RS}) follows: Assume

$$_s\dot{E}_R = a/_st_r. \quad (1)$$

Define

$$R_{RS} = 1/_st_r. \quad (2)$$

Then, from (1) and (2),

$$_s\dot{E}_R = aR_{RS}. \quad (3)$$

By definition and assumption, the difference between the short delay and long delay momentary effective excitatory tendencies is

$$_{s_s}\dot{E}_R - _{s_1}\dot{E}_R = a_sR_{RS_s} - a_1R_{RS_1}. \quad (4)$$

Since the constant, a, is intended to account for the use of different organisms, different instrumental responses, or different units of time, and since all of these variables are identical in the present experiment, it follows that

$$a_s = a_1. \quad (5)$$

Then, from (4) and (5)

$$_{s_s}\dot{E}_R - _{s_1}\dot{E}_R = a(R_{RS_s} - R_{RS_1}). \quad (6)$$

Further, by assumption,

$$\text{Mean } _s\dot{E}_R = _s\bar{E}_R - 2.5\,\sigma_0, \quad (7)$$

and since the standard deviation of oscillation is assumed to be constant for the same animal, from (6) and (7), differential excitatory tendency estimated from the response speed data

$$D_{RS} = _{s_s}\bar{E}_R - _{s_1}\bar{E}_R = a(R_{RS_s} - R_{RS_1}). \quad (8)$$

Per cent choice of the short delay bar is defined as the proportion of the free trials on which the subjects responded to the short delay bar. Hull has assumed that one can predict the per cent of times one response will be

chosen over another, given the value of the two competing excitatory tendencies, by calculating the probability that one random ${}_s\bar{E}_R$ from one normal distribution will be larger than a random ${}_s\bar{E}_R$ from the other distribution. This procedure can be worked backward, using the psychological scaling technique developed by Thurstone, calculating from an obtained per cent preference an estimate of the difference in units of the standard deviations of the two distributions.

This estimate is derived mathematically as follows: Define x_C as the abscissa of the normal probability integral (with zero mean and unit variance) at which the ordinate equals the proportion of times one response is chosen over another. If the two distributions are normally distributed, as assumed by Hull, then

$$x_C = ({}_s\bar{E}_{R_2} - {}_s\bar{E}_{R1}) / \sqrt{\sigma_s^2 + \sigma_1^2 - 2r\sigma_s\sigma_1}. \quad (9)$$

Assuming Thurstone's Case V, in which σ_s equals σ_1, and r is zero[5]

$$x_C = ({}_s\bar{E}_{R_2} - {}_s\bar{E}_{R1})/\sigma\sqrt{2}. \quad (10)$$

By transposition, and letting

$$b = \sigma\sqrt{2}, \quad (11)$$

$$D_C = {}_s\bar{E}_{R_2} - {}_s\bar{E}_{R1} = bx_C. \quad (12)$$

Preferential latency is defined by counting the per cent of times in each block of four trials (of which the first is free and the last three forced, but in which the second trial is forced to one side and the fourth is forced to the other side) the short delay response speed (on the second or fourth trial, as applicable) is greater than (or the latency less than) that to the long delay bar. Any such individual response speed represents (by assumption) one random selection from the response speeds possible at that given level of ${}_s\bar{E}_R$. This, as with the derivation presented for per cent choice, gives an empirical estimate of the probability that one random ${}_s\bar{E}_R$ from one normal distribution is larger than a random ${}_s\bar{E}_R$ from the other distribution. By the same method of derivation, but defining x_{PL} as the abscissa of the normal probability integral (with zero mean and unit variance) at which the ordinate equals the proportion of times the response speed to the short delay bar is greater than that to the long delay bar,

$$D_{PL} = {}_s\bar{E}_{R_2} - {}_s\bar{E}_{R1} = bx_{PL}. \quad (13)$$

It should be noted that the constant b is identical in equations (12) and (13), being dependent upon the standard deviation of oscillation which, because the same animal is involved, will be equal in either calculation.

Several difficulties are encountered in discovering the relationship between these estimates. Response speed and preferential latency are measured from the same observations, obtained in the forced situation where but one bar is presented and illuminated. Choice, however, is obtained in the free situation where both bars are presented and illuminated. The stimulus conditions differ, therefore, between choice and the other two measures. Further choice is always obtained from the first trial of a block of four trials, and this measure is thus *constantly* displaced in terms of the number of previous reinforcements. Since response speed is considered on the second and fourth trials, and since half of the time the short delay response is second and half of the time it is fourth, mean response speed is counterbalanced within itself for order. However, preferential latency, comparing second and

[5] Mosteller (3) has shown that this is the least squares solution when r is constant, not necessarily zero as assumed by Thurstone.

fourth latencies, is *not* averaged, but counted in the order given, and the effects of order may favor the fourth latency in any given block of four trials.

It should be recognized that these discrepancies are not simply artifacts of the present experimental design, but appear to be necessary difficulties. Forced trial latencies must be obtained on different trials, and each must be different from the free trial situation. The obtained relationships to be presented, therefore, should be considered in the light of these obstacles.

Let us first consider the relationship between the estimates of D_{RS} and D_{PL}. These two estimates of differential excitatory tendency are obtained from the same forced trials, and the only difficulty is that order may affect preferential latency while it is averaged out by counterbalancing in response speed. This relationship is afforded by setting up two paired distributions, one of the short delay response speeds and the other of the long delay response speeds, by blocks of four trials to each bar. D_{RS} is obtained by finding the difference between the means of these two distributions. D_{PL} is obtained by counting the number of times, of the pairs of short and long delay response speeds, the response speed to the short delay bar is the greater. On the assumptions that the distributions are normal, that there is no correlation between the particular short delay response speed and the particular long delay response speed falling in a pair, and that the two variances are equal and constant, these two difference estimates should, over calculations in all blocks of four trials to each bar, be linearly related.

This follows from (8) and (13),

$$bx_{PL} = a(R_{RS_s} - R_{RS_l}) \qquad (14)$$

$$x_{PL} = a/b(R_{RS_s} - R_{RS_l}). \qquad (15)$$

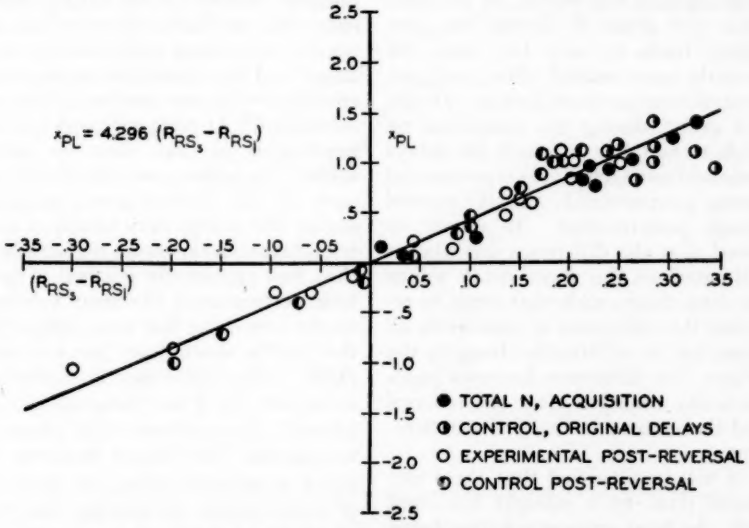

FIG. 1. Scatter-plot of estimates of differential excitatory tendency based on response speed and preferential latency.

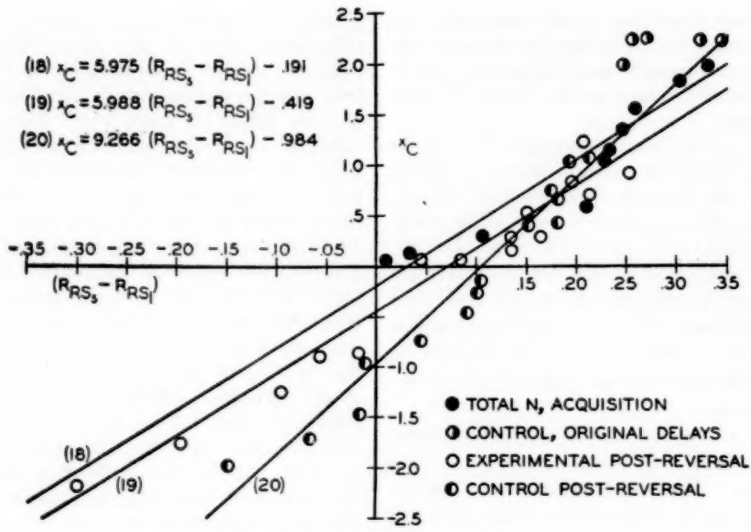

FIG. 2. Scatter-plot of estimates of differential excitatory tendency based on response speed and per cent choice.

The obtained empirical relationship is presented in Fig. 1. Calculations were made in four regions of the total data: (1) total N during the first eighty trials to each bar, since all animals were treated alike, and performance curves were similar; (2) control group during the additional 64 trials to each bar on which the delays remained unchanged; (3) experimental group, post-reversal; and (4) control group, post-reversal. It should be noted that the difference was always estimated as the short delay minus the long delay, such that prior to reversal this difference is positive in all cases, but by arbitrarily changing the delays, the difference becomes automatically negative after the reversal and then moves in the positive direction.

It will be observed that these empirical data fit a straight line very well, the least squares solution being

$$x_{PL} = 4.296\,(R_{RS_3} - R_{RS_1}) - .04. \quad (16)$$

This fact tends to substantiate the assumptions that response speed is linearly related to excitatory tendency, that oscillation distributions are normal with equal and constant variances, and that particular momentary effective excitatory tendencies are uncorrelated. At odds with one specific implication is that when an intra-subject variance was calculated for each of the control group subjects during the last twenty blocks of four trials before the reversal, during which they had apparently reached asymptotic performance, the mean variance to the long delay bar was .0336 while that to the short delay bar was only .0202. This difference is significant at beyond the 2 per cent level of confidence. The goodness of fit obtained to equation (16) should therefore be taken as substantiating the total set of assumptions, recognizing that discrepancies may occur in any particular assumption but are camouflaged by

deviations from the other particular assumptions and by the general variability of the data.

Since the same set of assumptions underlying the linear fit of the response speed and preferential latency data have also been made for the relationship between response speed and per cent choice, the fact that an adequate fit to equation (16) was obtained suggests that errors in these assumptions should not generate greater difficulty in the plot of choice against response speed. However, it should be recalled that choice is always obtained on the first trial of each block of four trials, and therefore has a constant displacement from response speed obtained on the second and fourth trials; and that choice is obtained from a stimulus situation containing two illuminated bars, while response speed is measured when just one bar is presented and illuminated.

The expected relationship, from (8) and (12), is

$$x_C = a/b\,(R_{RS_s} - R_{RS1}). \quad (17)$$

The plot of the two estimates of differential excitatory tendency, D_{RS} and D_C, is presented in Fig. 2. Calculations were made in the same four regions of the total data as in Fig. 1. It will be observed that three sets of points each appear to yield a reasonable fit to straight lines, but that the three sets of points do not suggest a fit to the same straight line. A least squares solution has therefore been obtained for (1) total N pre-reversal, (2) experimental post-reversal, and (3) control post-reversal. These three equations are, respectively,

$$x_C = 5.975\,(R_{RS_s} - R_{RS1}) - .191, \quad (18)$$

$$x_C = 5.988\,(R_{RS_s} - R_{RS1}) - .419, \quad (19)$$

and

$$x_C = 9.266\,(R_{RS_s} - R_{RS1}) - .984. \quad (20)$$

The small negative constant in (18) re-

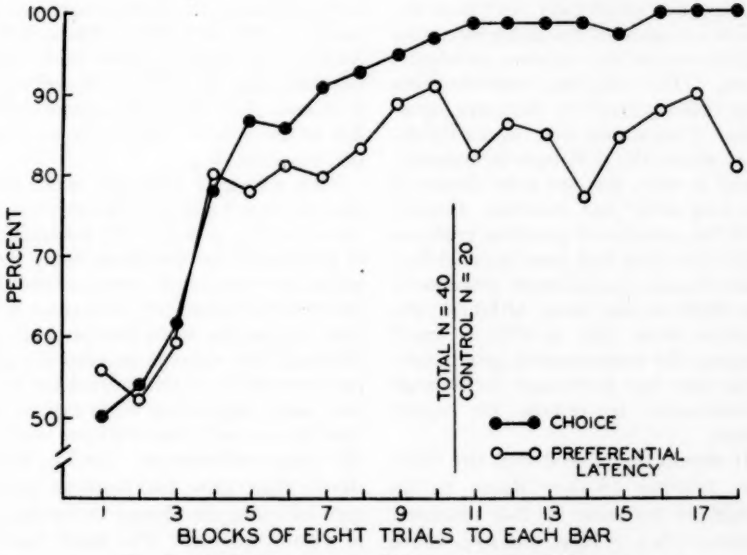

FIG. 3. Acquisition per cent choice and preferential latency.

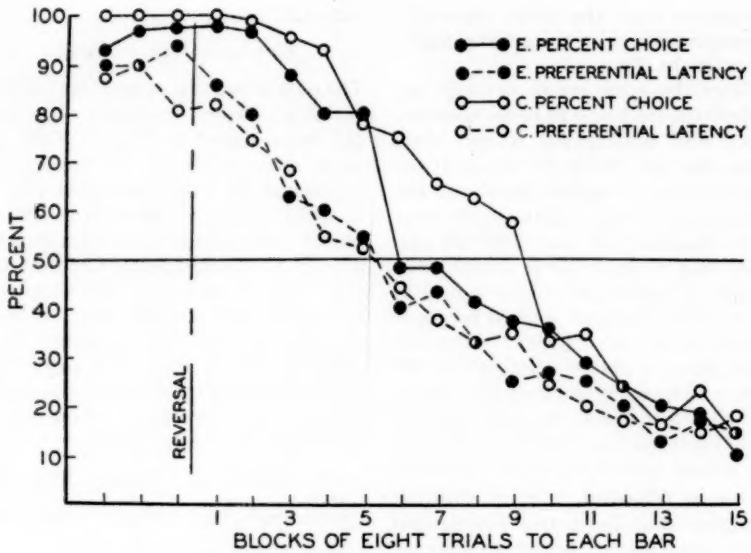

FIG. 4. Post-reversal per cent choice and preferential latency.

sults, at least in part, from the fact that the choice data were obtained before the response speed data, such that the choice estimate of the difference runs slightly behind the response speed estimate. This intercept, however, gets progressively larger in the three equations. This shows that, under conditions where the difference in response speed is zero, the per cent choice of the *long* delay bar increases directly with the number of previous trials on which that bar had been short delay. There is also a progressive increase in the slope of the lines, although the increase from (18) to (19) is small because the experimental group subjects were not continued far enough post-reversal to obtain the upper points.

It should be recalled that the function relating $(R_{RS_2} - R_{RS1})$ to x_C should be the same as that obtained between $(R_{RS_2} - R_{RS1})$ and x_{PL}, where a reasonable fit to a single straight line was obtained. The slope of (18) is considerably greater than that of (16), and this discrepancy gets progressively larger in (19) and (20). Thus, if the constant in (16) is taken to be the best estimate of a/b in (15) and (17), it is seen that even the approximate fits to linearity in Fig. 2 fail to have the expected slopes.

This difficulty becomes most evident in Figs. 3 and 4. The solid circle curve of Fig. 3 shows the acquisition of preference for the short delay bar, while the open circle curve shows the preferential latency of these same subjects during the same blocks of trials. Although the animals developed a 100 per cent choice of the short delay bar, they were responding more rapidly to this bar on only about 90 per cent of the compared latencies. In Fig. 4 are shown these same two response measures following the change in the delays of reinforcement. The solid curves represent the change in preference of

the experimental and control groups, falling from about 100 per cent choice of the originally short delay bar to 90 per cent of the new short delay bar. The shapes of these curves suggest that rats will develop a 100 per cent choice of the short delay bar under either original or reversed delay conditions, provided sufficient trials are given prior to and after reversal. The post-reversal preferential latency, shown by the dashed lines in Fig. 4, begins at a pre-reversal asymptote of about 90 per cent preferential latency of the originally short delay bar, and appears to reach a post-reversal asymptote of about 90 per cent preferential latency of the new short delay bar. It is apparent that these two measures are not identical, as expected from a theoretical analysis, although the general trends are similar.

These findings suggest the difficulty to be encountered when a theory devised on the basis of classical and simple instrumental conditioning is applied to a somewhat more complex learning problem involving two competing response tendencies. It would appear as if the resolution probably follows from the fact that the choice data were obtained in a situation containing two stimuli, while the forced response speed data were obtained in a situation where but one stimulus was presented. Apparently patterning, or neural interaction, must be brought to bear on the choice situation. However, because the post-reversal relationship between choice and response speed differed between the experimental and control groups, it would appear as if this patterning must indeed be a function of experience in the choice situation and not immediately determined by the presentation of the two stimuli simultaneously.

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PERCEPTUAL DEFENSE

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Within the last decade or so there has been developing a very promising movement to review and revise the experimental study of perception in the interests of a functional view of perceptual processes as instrumental in the services of the adjustive functions of the organism. The movement has, in the main, arisen from a growing dissatisfaction which those interested in clinical, social or personal problems have felt with classical Gestalt treatment of perception. This appears to them "as a kind of self-sufficient encapsulation of the perceptional system" (**6**, p. 56), that is to say, a formalistic study of perception *per se* treated as detached from the needs, attitudes and adaptive styles which govern the person's commerce with his environment. As opposed to this "encapsulation" the "new-look" functionalists are concerned "with the manner in which perceptual process is embedded in and interacts with other forms of psychological functioning" (**2**, p. 15) as, for example, values and needs, reactions to stress or threat or more general characteristics of personal life styles.

In bringing into the field of laboratory experimentation conceptions and problems which have, for the most part, lain outside it, the movement is quite exciting in its promise of exploring with rigorous laboratory techniques matters which have for so long appeared as if they could only be discussed in a near-literary technique of cumulative suggestion by illustrative instance and analogy rather than in terms of the systematic examination and verification of hypotheses of accepted scientific procedure. Although as yet the findings and theorizings are quite tentative, offering stimulating suggestions rather than final demonstrations, and although the experimental procedures are open to criticism, e.g., (**17**, **21**), the movement of functionalist perception has already got sufficiently under way to raise challenges which, whatever the outcome, cannot but revitalize the study of perception. And this is not merely a matter of renewed activity in this area of inquiry. Something of a renaissance with the most interesting possibilities for systematic psychological theory is evidenced in the meeting of diverse interests on the common ground of the study of perception which, as Werner points out, has the dual advantages of "precise manageability in experimental design" and "nearness to life." "It is this twofold characteristic, not obtained by any other psychological function, that is rapidly making the area of perception a meeting ground of general experimental and clinical-social psychology" (**28**, p. 3). But if the meeting ground is not to be just a battle ground, it would appear necessary that the parties must give up something of the predisposing sets they have brought from their particular interests. It is likely that certain notions tested on this common ground cannot escape revealing their inadequacy. One such notion is, I believe, that of perceptual defense, implicit or explicit in the theorizing of a number of the perceptual functionalists, and specifically and whole-heartedly championed by Postman, Bruner, and their associates.

The widespread influence of the defense notion may be indicated by reference to contributions to the 1949 *Journal of Personality* symposium. From a personalist viewpoint, Klein and Schles-

inger claim that individual variations in the absolute and difference thresholds of the psycho-physical methods may "reflect not chance variations or experimental errors but express controlling forces of integration such as defenses" (14, p. 37). These authors, further, cite evidences of relative deficiency on the part of neurotics in sensory acuteness and discrimination (7, 19, 25) as supporting an interpretation in terms of neurotic flight from reality extending into functions usually regarded as sufficiently peripheral matters of individual variation in sensori-neural capacities. In a psychoanalytic framework Else Frenkel-Brunswik (8) interprets lack of alertness to changes in a perceptual situation as involving an incapacity to tolerate ambiguity, a clinging to the familiar and clear-cut by way of protection against disruptive pressures of internal conflict arising from the ambivalent nature of child-parent relationships. In this symposium also Bruner and Postman interpret the difficulty of breaking a dominant set as evidencing perceptual defense (4) and, in a review of the work of what may loosely be called their "school," formulate perceptual defense as a basic axiom: "Given a stimulus input of a certain character, perceptual process operates to maximize current needs and expectations and to minimize percepts inimical to such needs and expectations" (2).

The above examples indicate that the perceptual defense notion is entertained in a variety of different approaches. I shall, however, direct my criticisms more particularly at the Postman-Bruner formulations because in these the concept is more explicitly developed.

The "minimax" axiom is also stated as dual principles of resonance and defense: resonance, in that a subject will tend to select from a stimulus situation that which fits his prevailing interests, or in an ambiguous situation will attempt to organize the situation "in a manner congruent with his prevailing set" (2, p. 88); defense, in that a subject will tend to warp or distort his perception of a disturbing situation to give it a meaning he finds less disturbing or, failing this, may give a meaningless response, or show blockage of response as if he were resisting any perception at all (1, 4, 18).

It is obvious that for perceptual process governed only by principles of maximizing and minimizing, resonance and defense would be maladjustive; an organism with such perceptual equipment could not survive the dangers it must perceive in order to meet them. On the evidence of PGR. records during the pre-recognition phases of attempts at perceiving emotionally toned, sexually tabu words, McGinnies postulates a principle of "sensitization to threat" (18) and, more generally, to cover these and other observations Bruner and Postman advance a principle of perceptual vigilance. In this notion we have an attempt to incorporate within the theory of perception a reality principle without which resonance and defense would make veridical perception impossible and, confining the person within his economy of need determinations, would deny him any effective adjustment to conditions other than his wishes or his fears.

Luchins points out the practical difficulties of these three principles for experimental inquiry: "Where does the process of resonance cease and the process of defense begin? and at what point does defense give way to vigilance?" (17, p. 75). These difficulties are certainly real difficulties of the kind that have hindered efforts at precise experimental check on Freudian principles (24), but objection on these grounds alone is not final. An interest in securing methodologically elegant formulations may itself be something of a defense against the

possibility that complex matters may require complex formulations and these may be of a dialectical kind. Further, the apparent opposition between the principles, as that resonance would be set in opposition to defense or vigilance, or that vigilance would be in opposition to defense, though bothersome, does not constitute a sufficient *a priori* objection; similar apparently contradictory principles are to be found in a variety of areas of research. To take but one instance, learning theorists have used opposed principles of facilitation and inhibition, and of these we might ask questions of the kind Luchins asks about the Bruner-Postman principles. There does, however, appear to be this difference about the Bruner-Postman principles: They do not make it possible for an investigator to determine from the concrete data where one principle gives place to another. As they stand, the principles of resonance, defense and vigilance do not permit of clear functional demarcation and rigorous testing, and there is, too, a smack of "saving hypotheses" about them. Vigilance, particularly in its aspect of sensitization to threat, appears to be brought in to meet the ultimate solipsistic difficulty of resonance and defense-determined perception. There is possibly a certain aptness here in Hochberg and Gleitman's acid comment on need-centered theory: "If one postulates that needs affect perception, there is always an extra need that can be called upon if the experiment should yield more complicated results" (**11**, p. 181).

Nor does it appear that the complications added to the explanatory formulation by the principle of defense are warranted by any necessary constraint of the data. I would agree with Luchins that what has been looked upon as calling for an interpretation in terms of defense may more simply and more profitably be regarded as an outcome of conditions of relative set-dominance. Luchins (**17**) found that both in a problem-solving situation and in a perceptual situation initial sets operated to hinder more appropriate reactions to changing circumstances. But in neither of these instances did he find it necessary to appeal to a principle of defense, nor did the subjects' introspections support such an interpretation.[1] If interpretation in terms of perceptual defense is inappropriate in these instances, it may reasonably be asked whether the observations of similar rigidities or lag effects reported by Klein and Schlesinger (**14**), Bruner and Postman (**4**) or Frenkel-Brunswik (**8**) any longer demand such an interpretation. So, too, what would appear to be the most challenging experimental evidence for perceptual defense, *viz.*, McGinnies' study (**18**), is quite consonant with a relative set dominance viewpoint. A given stimulus situation is presented under various possibilities of consideration by the person, i.e., its various aspects may be perceived or discriminated at various levels as governed by the varying set dominances of the individual.[2] Thus in relation to one type of set dominance, the tabu

[1] Luchins' reference to subjects' introspections is interesting. The implication is that notions of defense do not present themselves in any direct phenomenological fashion. Support is lent to this conclusion from studies aiming at a systematic introspective analysis of processes of volition and choice. Thus Wells (**27**), for example, does not report cases of subjects seeing themselves protecting a set to one line of choice by defending against another.

[2] It is to be noted that Bruner and Postman (**2**) discussing the apparent paradox of perceptual defense—that somehow the subject must know the stimulus in order to avoid knowing it—argue that the stimulus situation may set off a variety of responses with differing thresholds. This is quite in line with the above discussion, but clearly it involves a shift in status for perceptual defense. Instead of a basic principle of perception, it has become something to be explained away.

words may be discriminated merely in their aspect of disturbers (as indicated by the PGR. records); under subsequent tachistoscopic exposures, in relation now to changed conditions of set, they may be recognized as distinct words with their explicit meanings. The delay in recognition of the tabu words as against the neutrally toned words points to a continued dominance of an affective set which hinders the stimuli being seen in the changed perspectives which make possible their explicit recognition. It would appear that what has been ascribed to perceptual defense can be seen as the consequence of the dominance of certain sets to the necessary exclusion or limitation of opposed sets, opposed not in any sense of fighting it out but simply in the sense that you cannot both have your cake and eat it. As Lewin puts it: "Each change in the psychological situation of a person means just this—certain events are now 'Possible' (or 'impossible') which were previously 'impossible' (or 'possible')" (**16**, p. 14).

It is argued, therefore, that the defense principle is methodologically unpromising, involves some buttressing with *ad hoc* principles, and is not in any necessary and inevitable way dictated by the data adduced in its support. These are serious criticisms, but even if they could be in large part met, there remains what to me is the most serious criticism of all. To speak of perceptual defense is to use a mode of discourse which must make any precise or even any really intelligible meaning of perceptual process impossible, for it is to speak of perceptual process as somehow being both a process of knowing and a process of avoiding knowing.

If, then, the principle of perceptual defense has so little to recommend it, the question arises, Why should it show such vitality, why should it prove so insidiously attractive? The answer is, I believe, that there is a peculiar liability to such interpretations on the part of those whose interests are primarily personalistic and, more particularly, clinical. In the attempt to explain personal functioning there are two related sources of confusion: (1) the interpretation of processes in terms of agency concepts, (2) the internalism of an exclusive emphasis on impulse.

For the first source, it may be appropriate in certain circumstances and at a certain level of analysis or description to speak of the person as an active agency, a seeker of ends, an avoider of ills. I am not raising the question of the legitimacy or limits of such approaches. The confusion lies in speaking of the part processes of the person as if they themselves were somehow persons. It is one thing to say that a person may defend himself against perceptions. He can, of course, do that quite simply by shutting his eyes or stopping up his ears. It is quite another thing to claim as an integral principle of perceptual process a principle of perceptual defense. This is to commit a category mistake in representing "the facts of mental life as if they belonged to one logical type or category (or range of types of categories) when they actually belong to another" (**23**, p. 16). Ryle has penetratingly shown us the stubborn confusions in our thinking about mind for which such errors are responsible.

With regard to the second source of confusion, by internalism I mean the tendency to explain behavior on a basis of organic impulse in fundamental separation from external influences. Naively we do quite generally feel that a sufficient explanation of our behavior is afforded when we relate it to an impulse, and an elaboration of this common sense principle was the characteristic of earlier instinct theory, notably that of McDougall and, in its very different way, that of Freud. To the degree that the emphasis is on impulse, as a consequence

of this particular set dominance, influences other than impulsive are likely to appear as alien and intractible. This, I think, holds for much clinical theorizing as it holds for our own immediate personal experience. Dealing so much with extreme cases of emotional breakdown, it is difficult for clinical workers not to see feeling and emotion as the phenomenal expression of the inwardness of need in sharp distinction to the cognitive processes which are more nearly related to the effects of external stimulation. Elsewhere I have argued that one aspect of this dichotomy, the emphasis on "feeling release," has operated to obscure interpretation of the dynamics of therapy (13). Here I would suggest that the too-sharp severance of feeling and knowing lies behind the attempts to bring defense into the theory of perception. It is, of course, in Freud that we find internalism carried to its furthest limits where the emphasis on the clamant demands of impulse has led to a principle of economy as a root principle of motivation: The organism is basically driven by a need to protect itself against stimulation, to defend itself against perception.

I suspect that the Bruner-Postman principle of perceptual defense is a direct offspring of Freud's economy principle and, so far as this principle is given primacy, it carries the same unsatisfactory consequences in that its logic leads to an ultimate solipsism of each in his own system of need-determined perceptions (12). To escape this absurdity, some sort of reality principle, the operation of constraints by the actual, has to be accepted ("the most adaptive response is frequently the one that takes most vigilant account of reality" [2, p. 94]), but this is acceptance rather reminiscent of Carlyle's comment on the lady lecturer who accepted the Universe: "Gad! she better had." For Bruner and Postman the reality principle is rather an alien and intractable principle. To the degree that the influence of needs and of the objective character of the world are not brought together effectively in a comprehensive theory of the organism's adjustive commerce with its environment, we are left with a stubborn dualism reflected in such antinomies as defense versus vigilance (2), distortion versus selection (21), behavioural determinants versus autochthonous determinants (1), unstructured or equivocal situations versus structured or unequivocal situations (2) —all various facets of the one central antinomy, economy versus adjustment.

Unquestionably the entry of need-centered theory into the area of experimental study of perception, till lately almost a close preserve of situation-centered theory, has performed the signal service of forcing these antinomies into the open. This gives us reason to hope that, when extreme positions are readjusted in the light of their experimental implications, a reconciliation of apparent divergencies will result. So far as present tendencies indicate, it is likely that the *rapprochement* will be along a line something of the kind suggested by Krech (15), a line where the impulsive and emotional will be seen not in restriction to their inwardness and insulation of felt disturbance and release, but rather as particular aspects of more general characteristics inherent in the dynamics of perceptional field forces "a . . . Dynamic System which is coordinate with our experience when we look at a 'lopsided' figure on a screen will be *under the very same kind of tension* as a Dynamic System which is co-ordinated with our experience when we look at a steak after several hours of food deprivation" (15, p. 79). This deliberately provocative illustration, however, suggests a danger in that neo-Gestaltism such as Krech's, drawing on principles of configuration like "closure" or "good fit" from classical Gestalt studies of percep-

tion of the kind Brunswik describes as "from the retina outwards" (5), may, in its emphasis on the immediate total situation, blur real distinctions. Thus Tolman finds it necessary to "continue to search for basic drives and continue to seek to analyze relatively enduring belief and value structures as something different from and obeying different laws from immediate fields and their dynamics . . ." (26, p. 369). So, too, Murphy speaks of a definition of perception in bi-polar terms: "We have been working our way slowly toward a definition of perception in bi-polar terms, as an organized process which is organized in terms of saliencies both of the world without and the world within" (20, p. 353).

Though the form of statement in the above quotation is somewhat ambiguous, read in the context of his general discussion, Murphy is not implying a bi-polarity in the functioning of perception, but a distinction between fields or contexts in which it occurs.

Perception does not oscillate between being one type of process of a cognitional kind in experiencing the external, and another type of process of an emotional kind in experiencing the internal. A given situation appears under twofold aspects: (1) as an aspect discriminated from other situations by its common inter-subjective denominator and (2) as an aspect which has relevancy for one or more needs of the organism. The complex possibilities of the organism's considerations of a situation cannot be any simple matter of perception somehow immediately occasioned by the mere occurrence of the stimulus pattern if this stimulus pattern includes both "inner" and "outer" reference. To cite Murphy again: "Whatever else perception may be, it is certainly an integrated act that involves the interrelationships of components and requires the study of modes of interrelation" (20, p. 354). In other words, it is a learned achievement.

We incline to attribute a fundamental directness and immediacy to perception because we fail to realize sufficiently that behind what may seem in the adult's perception a simple given there lies a long and complex process of learning,[3] particularly learning in the ever-present process of social training, incidental or designed. And we are particularly apt to ignore the degree of learning about our needs and our feelings as well as about external circumstances, and, further, the degree of learning which is involved in relating what we have learned of our needs to what we have learned of our circumstances. At one level or another, even an apparently simple situation would involve for its appreciation by the individual, not only discrimination of its external or objective characteristics, but also discrimination of the interrelationship of his own needs. For a young child to perceive the dinner bell means that he has to distinguish one sound from other sounds; he has to relate it to his need to eat or he may not hear it, since he may be engrossed by another need, the need to play; he has, further, to discriminate between needs; otherwise he may hear it in relation to his play, as, say, the bell on the fire engine. Of course it is not suggested that the child in this instance has to put himself through a complicated process of self-analysis. Normally the appropriate perceptual discriminations occur immediately and effortlessly because they have been well learned. Considerations of characteristics of the final achievement should not, however, cause us to ignore the possibilities of long continued and even laborious learnings which have brought it about.

The point of this for the problem of perceptual defense is that, so far as we are led to see perceptual process as other than a process that makes possible the

[3] Hebb's book (10) should prove a valuable corrective of this tendency.

knowing of what at various levels are complex matters, we are involved in unbridgeable dichotomies or misleading simplifications. As a process of coming to know complex matters it must involve complex learnings, and thus will be subject to all sorts of errors. But unless even the errors are looked upon as the result of efforts to know and not as the outcome of defenses against knowing, there appears no possibility of bringing within our perceptual resources a reality principle in terms of which we may understand adjustment as something more than the shifts and subterfuges of a basic drive to avoid stimulation. Though it may appear a far cry from the sober discussions of Bruner and Postman to the near mysticism of Freud's *Beyond the Pleasure Principle*, I believe his grim picture is the logical outcome of the defense notion. Adjustment, intelligent behavior and achievement become but "more complicated and circuitous routes to the attainment of the goal of death" (9, p. 48), so that the organism does not develop in any positive way but like Charles II takes "an unconscionable time dying" because it "is resolved to die only in its own way" (9, p. 48). Apart from such imaginative flights, is it too unkind a suggestion that in the degree that it clings to a notion of perceptual defense the Bruner-Postman theory of perception "is resolved to die only in its own way"?

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A NOTE ON ONE-TAILED AND TWO-TAILED TESTS

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The following remarks were provoked by the recent paper by Marks [1] on this subject. Although the impracticability, in most cases, of deciding just what level of significance is "significant" makes it superfluous to fuss about minor inaccuracies, the difference between one-tailed and two-tailed tests can be serious. Moreover, the problem of which to use in a given case does give an appreciable amount of trouble to the research worker. It appears to me that the interpretation given by Marks errs on the unsafe side.

In the first place, it should be clearly understood—though Marks, perhaps inadvertently, implies the opposite—that it makes no difference *when* a theory or hypothesis is conceived, as far as its logical content and implications are concerned. Logic is timeless; it does not, and cannot, matter whether a theory was conceived before, during, or after the experiment; it may be suggested by the data, or it may be revealed in a dream. A hypothesis derived from the data may be expected to fit the data, more or less; but the statistical calculations are intended to show how well or how badly it does fit, and, in the vast majority of cases, to show this on the assumption that personal opinions and collateral evidence are set on one side.

This is quite separate from the subtraction of degrees of freedom when parameters of the chance hypothesis are calculated from the data. When we do this, we are not depending on the data to provide the chance hypothesis, but to tell us which of a certain family of such hypotheses will act as the most stringent test. Ideally, we should choose the hypothesis which best fits the data, with the exception that it must have no preferential tendency to produce the effect in question—e.g., a deviation of the mean, or something of that kind. If we can exclude this chance hypothesis, then we can *a fortiori* exclude all others, since they are less likely. This is the step which requires us to deduct degrees of freedom, because it is only this step which gives us any appreciable amount of information from the data. To be able to reject one chance hypothesis tells us nothing, since there is an infinity of others; but to be able to reject a whole class really means something, and the data must pay.

However, though robbing the data in this way affects the statistical significance, it does not appear to have anything to do with the problem of one-tailed and two-tailed tests. To deal with this, we must first consider the statistical question asked—namely, "Could my results reasonably be due to chance, or must I assume that they were due to some cause or systematic bias?" Or, since the experimenter is not necessarily required to make up his mind on this point, he may go no further than the question, "What is the probability, on the chance hypothesis, of my results occurring?"

Now this, unfortunately, is just what the ordinary statistical methods fail to answer explicitly. As is well known, the probability of the particular sample depends, not only on the shape of the distribution, but also on the number of possible alternative samples. On the other hand the P of the ordinary significance

[1] M. R. Marks, Two kinds of experiment distinguished in terms of statistical operations. PSYCHOL. REV., 1951, **58**, 179–184.

test is, strictly speaking, a probability irrelevant to the question of whether the chance hypothesis should be rejected on the grounds of unlikelihood. The best we can say of it is that it happens, in the ordinary cases, to be vaguely related to what we want to know.

If the reader doubts this, he has only to consider what "significance" he would attach to the occurrence of an observation in, say, a 5 per cent slice at the end of a rectangular distribution. Obviously such an event would not give us the least grounds for rejecting the hypothesis, and, in fact, there is no significance test, in the ordinary sense, for this distribution, except for the trivial case of an observation right outside it. This is almost a *reductio ad absurdum*, but it serves to emphasize that P, though it is a probability, is not the one which, by itself, makes us view the chance hypothesis with suspicion.

In the Neyman and Pearson theory of critical regions P has a definite place. But some statisticians think this theory approaches the problem from the wrong end. It is certainly difficult, and often impossible, to apply. If then we are compelled in practice to use some rough and ready method, which is the least rough and the most ready? It is clear that the actual sample probability is what matters, and it is possible to make some sense of it by dividing it by the probability of the most probable sample. This relative probability answers the question, "How much less likely are my results than what I should have expected on the chance hypothesis?" However, this also leads to absurdity in special cases, and we must admit that there is no easy method that is quite foolproof.

The essence of the ordinary P, correctly used, is that it is the class probability of a critical region composed solely of all the samples whose individual probabilities are below a certain critical level. The distribution may go up and down like a mountain range, but wherever the curve falls below this level, there is a piece of the critical region in that place. This is not the case, incidentally, for the tabular probabilities of Chi^2, which are only the probabilities of *exceeding* a given value. Marks states that these probabilities are for two-tailed tests, but this is only true in the rather different sense that they refer indiscriminately to positive or negative discrepancies. But the Chi^2 distribution has a short tail as well as a long one, and both should properly be included in the critical region. As it is, we just have to remember that small values are "suspicious."

In attempting to justify the one-tailed test, Marks appears to regard as equivalent such states of mind as *knowing* that, say, a negative deviation cannot occur except by chance, not being interested if it does, guessing that a positive deviation will occur, and so on. What the experimenter is interested in has nothing to do with the case. What he guesses is relevant, if he can show that it is a rational guess and can assess it in terms of probability. What he knows for certain is still more important. If he knows that a negative bias is impossible, it means that no negative deviation, however great, will shake his faith in "pure chance" as the cause of it. If he is in as strong a position as that, of course he is entitled to put the whole critical region in the positive tail and call it a one-tailed test. What he is doing is to accept a more lenient degree of significance, though it is difficult to see why he should bother with this form of test at all; if the bias cannot be negative, there is little risk of it being exactly zero.

But suppose he is only moderately sure that the bias is positive; is he to do a one-and-a-half-tailed test, or something like that? By all means, if he can assess his prior knowledge in terms of

fractions of a tail, except that it then follows that there are not just two kinds of experimentation, as Marks suggests, but one for every degree of prior expectancy.

It is far better to carry out the statistical work in the ordinary way, without tampering with the critical region, and let prior probability (or any other relevant consideration) determine what level of significance you will regard as decisive. You can still believe in a positive deviation more readily than a negative one, if you like; but you do not confuse the issue for others who may have different ideas.

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SOME REMARKS ON THE AMES OSCILLATORY EFFECT

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Ames has described in his recent monograph on the rotating trapezoidal window a number of important and interesting effects (1). Only one of the effects will be considered here for experimental analysis and theoretical discussion: the perception of an oscillatory motion when the trapezoidal window is actually rotating. The effect is quite compelling and easily reproducible. Ames interprets this effect along empiricistic lines. In general it reenforces his "assumptive" and "prognostic" approach to perception. The Ames demonstrations and approach have not gone without criticism. Pratt, in a recent commentary (4) on the work of Ames and of his followers, interpreted a few of the demonstrations in non-empiricistic terms. Pratt, however, did not touch upon the rotating trapezoidal window experiment. The object of this note is to apply Pratt's pattern of thinking to this latter demonstration.

Essential to Ames' explanation of the apparent oscillatory motion are: (1) the unequal lengths of the parallel edges of the trapezoidal window, (2) the *O*'s assumption (acquired on the basis of actions undertaken in a world of rectilinear forms) that the trapezoidal window is rectilinear. The present writer will show that (1) is not a necessary feature of the oscillatory effect and that (2) entails grave logical difficulties. Both of these points will be discussed in turn.

In the writer's own experiments which are to be described here, small figures cut out from oak tag paper were used. The effect described by Ames can be obtained with these reduced figures and they considerably lessen the amount of work involved in cutting the figures and installing them in the apparatus. The apparatus was purchased from the *Institute for Associated Research,* Hanover, New Hampshire.[1] Figures were placed against a homogeneous background and illuminated by two shaded 75 watt bulbs. The bulbs were symmetrically placed about eight feet away from the figure and were themselves eight feet apart.

(1) Ames' explanation implies that the perceived oscillatory motion is unique to the trapezoidal window. The writer, however, found that the oscillatory effect could be obtained with a wide range of forms. The following forms yielded this effect: circle, ellipse, lop-sided ellipse, triangle, rectangle, trapezoid, diamond, hemispherical arc, a *V* shaped cut-out, and finally a highly irregularly shaped cut-out. The extent and character of the oscillation usually did not coincide with the oscillation obtained with the trapezoidal figure. Variations in the position of the axis of rotation did not affect the appearance of oscillation. Furthermore, the effect was not confined to plane figures. The measurements of the non-planar figure which was used are as follows: rectangular base—5" by 1½"; the two end faces which were perpendicular to the base were respectively 2" and 1½" high; two parallel cardboard strips were pasted across the vertices of the two end faces. The axis of rotation was through a line perpendicular to the base at its center.

The effect was also obtained with a luminous rectangle and with a luminous trapezoid in a dark room.

Although the writer used smaller figures than did Ames, it is most likely that

[1] The $7.00 kit was used.

similar results will be obtained if larger figures were employed. Contrary to one of Ames' conclusions (1, p. 15), the oscillation effect was obtained with the rectangular window supplied wtih his kit. The discrepancy between this result and that of Ames may lie in the different times of exposure. Ames makes no statement about the duration of exposure to the figure. The writer found that for this particular figure a period of exposure (during which time the rectangular window was perceived as rotating) was necessary before perceived oscillation was obtained. Thirty of 58 *Os* tested in a lecture demonstration noted oscillation or reversal of motion after three minutes. The *Os* were placed thirty-five feet away from the figure and were asked to observe the motion of the figure. Otherwise they were uninstructed and were unaware of the fact that the figure always rotated in a clockwise direction. The effect was obtained with one eye as well as with two eyes.

The oscillation effect was also obtained with the trapezoidal window itself when it was placed on the vertical rod so that the parallel edges of the window were perpendicular to the rod.

Discussion of results. On the basis of the foregoing results it follows that that part of Ames' explanation of the oscillation effect which involves the inequality of the two parallel edges of the trapezoidal window is invalid. The oscillation effect was obtained with a wide variety of figures—rectangle, ellipse, circle, etc. Since the effect described by Ames is of a more general nature, an explanation is necessary which covers the available evidence and which permits verifiable predictions. The writer will offer an explanation after discussion of the next point.

On the theoretical side, Ames explicitly states that the past experience of the individual is an effective factor in producing the oscillation effect. Ames asserts that the individual assumes that the trapezoidal window is rectilinear. Ames then proceeds to develop an explanation of the effect. This explanation involves the inequality of the parallel edges of the trapezoidal window. Since the oscillation effect is quite general in nature Ames' explanation requires modification. As it is now stated, however, Ames' explanation does not fit the facts. There exists another difficulty in Ames' account of the effect. The empiricistic explanation advanced by Ames could not possibly account for his own results. Let us turn to the Ames monograph.

Ames states that the trapezoidal window is perceived as a rectangular form. According to him, this fact "means that the perceived rectangular form does not come from either the stimulus pattern or the object" (1, p. 14). Ames then raises the following question: "If it does not come from the stimulus pattern or the object, whence does it come?" (1, p. 14). Ames' answer to the question is as follows.

> "In his past experience the observer, in carrying out his purposes, has on innumerable occasions had to take into account and act with respect to rectangular forms, e.g., going through doors, locating windows, etc. On almost all such occasions, except in the rare case when his line of sight was normal to the door or window, the image of the rectangular configuration formed on his retina was trapezoidal. He learned to interpret the particularly characterized retinal images that exist when he looks at doors, windows, etc., as rectangular forms. Moreover, he learned to interpret the particular degree of trapezoidal distortion of his retinal images in terms of the positioning of the rectangular form to his particular viewing point" (1, p. 14).

But how could the individual, in his actions undertaken in an environment in which the retinal impression of rectilinear forms is most frequently nonrecti-

linear, form an assumption about rectilinearity which is claimed to be so effective in shaping the sensory material? Why not form an assumption of "trapezoidalness"? Since, from the Ames standpoint, the "assumption" formed by the individual represents a kind of weighted average of actions in conjunction with retinal impressions, it would seem to follow that the best assumption is that of some trapezoidal form (or more exactly, some trapeziform). Thus, on the basis of experience, or actions, or retinal impression, the preferred perceptual tendency to see rectilinearity remains unexplained. In answer to his question Ames should have considered the possibility that the preferred perceptual tendency is the outcome of the distribution of central processes (which are functionally related to retinal impressions) in accordance with principles advanced by Gestalt psychologists (*cf.* 3).

Since Ames' explanation is inadequate to account for the facts on both empirical and theoretical grounds the writer would like to propose an alternative interpretation which seems to be in closer accord with the facts and which also enables one to predict the occurrence of the oscillation effect. In the following discussion the various plane figures should be regarded from the point of view of *O;* the tilt or slant of a given figure will be measured by the angle that the plane of the figure makes with a frontal plane—the angle always being measured from the right half of the frontal plane to the plane of the figure in a counter-clockwise direction (the positive direction), or in a clockwise direction (the negative direction). Moreover, a distinction will be drawn between the *actual* tilt of a plane figure and the *perceived* tilt of a given plane figure. The actual tilt may or may not coincide with perceived tilt. It will be shown that oscillation (or reversal of the actual rotation of the plane figure) occurs when *O*'s perception of the tilt of the plane does not coincide with the actual tilt. It should be kept in mind that the actual rotation of a given plane figure is always clockwise.

Suppose that a given plane figure—for example, the trapezoidal figure,—is tilted at an angle of plus 45 degrees and that the longer edge of the trapezoidal figure is farther away from *O* than the shorter edge. The plane figure is now rotated. Let us consider the changes that take place on the retina. The retinal projection corresponding to the rotating figure will be an enlarging one until the plane figure is in a normal plane. If the rotation of the plane figure continues, then the corresponding retinal image will be a contracting one until the figure has rotated minus 90 degrees. Thus, as the trapezoidal figure rotates, there will be a cyclical expansion and contraction of the projected contour on the retina.

Let us now return to the original assumed position of the trapezoidal figure —*viz.*, at a tilt of plus 45 degrees. It so happens that the usual *O* will perceive the tilt to be not plus 45 degrees but some negative acute angle. That is to say, the long edge of the figure will be seen as being closer than the shorter edge. Despite the fact that the perceived tilt does not coincide with the actual tlit, the retinal projection of the rotating trapezoidal figure must remain an expanding one. In the phenomenal experience of *O* this could only be the case when the perceived direction of rotation of the trapezoidal figure is counter-clockwise. An alternative way of describing the same thing: Consider a set of points which are distributed over the plane figure. When the trapezoidal figure rotates from its original position of plus 45 degrees the movement of the projections of the points on the retina will be in opposed directions, with the vertical rod as the dividing line of the two divergent

motions. Those points on the trapezoidal figure to the right of the rod will move away from the rod to the right, whereas those points to the left of the rod will also move away but to the left. If *O* sees the actual tilt of the plane figure, he will then perceive the true clockwise rotation of the figure. On the other hand, if *O* perceives the tilt to be other than the actual tilt, the points on the figure will be seen on the perceived surface. Consequently, this new situation is equivalent to one in which the plane figure begins its rotation from a position of some negative acute angle. The points on the right side of the rod still move in a direction away from the rod to the right, and those points to the left of the rod move in an opposite direction. From this analysis it follows that the perceived movement of the plane figure will be counter-clockwise. Thus, an appropriate analysis of the Ames figure involves two components: movement of points across the retina and the observer's perception of tilt.

The writer's interpretation can also be used to explain the oscillatory effect which was obtained with the other plane figures. If *O* reports rotation instead of oscillation, it can be assumed that his perception of the tilt of the surface coincides with the actual tilt. Oscillation occurs when perceived tilt differs from the actual tilt. The explanation offered here can be experimentally tested. In the experiment to be discussed the small trapezoidal window was used.

The position of this figure was fixed at plus 45 degrees with the long edge away from *O*, who was then asked to report which edge appeared to be the closer one. When *O* gave his report *E* started the motor and permitted the window to rotate about 90 degrees. The following results were obtained. If *O* reported the long edge to be the closer one (perceived tilt is not coincident with the actual tilt), he perceived the motion in a counter-clockwise direction. If *O* reported the short edge to be the closer one (perceived tilt is coincident with actual tilt), he perceived the motion to be in a clockwise direction, which is the actual direction of rotation.

The explanation advanced here in this note can account for other facts which at first appear to be quite different from the Ames oscillatory effect but which are essentially of the same character. One of these facts will be chosen for discussion and the others will be mentioned.

If the concave side of the mask of a human face is observed from a distance of, say, fifteen feet, it is well known that most *O*s will report perceived convexity (2, p. 293). The effect is enhanced if shadow effects are introduced. If the mask is rotated in a clockwise direction (and *O* continues to report convexity), the perceived direction of rotation is counter-clockwise. This effect is compelling and consistently obtained. How is this effect to be explained? For purposes of exposition let us consider a series of points which are distributed on the concave surface of the mask. Suppose that the clockwise rotation of the mask is begun when plane of the mask is plus 45 degrees. The motion of the retinal projections of the set of points will be in a left to right direction. If *O* perceives convexity, the points on the concave surface are now seen on the convex surface. As the mask rotates, the points on the perceived convex surface still sweep from left to right. If the actual convex surface were being observed and the points on the surface moved from left to right, *O* would see a counter-clockwise rotation. In the phenomenal experience of *O* there is no difference between the perceived convexity of the concave mask and the perception of an actually convex surface.

Therefore, the phenomenal counter-clockwise rotation of the mask (when convexity is perceived) is equivalent to the perception of an actual convex surface rotating counter-clockwise.

The same interpretation can be applied to the inversion of rotation in a three-dimensional cube and to the similar inversion that one obtains upon fixating the concave side of a solid angle.

One point of the interpretation proposed by the writer which remains unexplained concerns the factors responsible for a perceived surface whose tilt differs from that of the actual surface. Since the tilt of the perceived surface can be checked behaviorally one can still utilize this fact in accounting for other phenomena. Perhaps the basis for the tilt of the perceived surface lies in the asymmetrical retinal projection of a given figure and the central processes which are innervated by the retinal projection. Such central processes may attain a more appropriate equilibrium when a given figure is perceived in other than its true orientation (*cf.* 3).

Summary. The oscillation effect described by Ames was shown to be of a more general nature than considered by him. Furthermore, it was indicated that Ames' empiricistic explanation was inadequate to account for the facts. The writer has proposed an alternative explanation.

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TOWARD A GENERAL THEORY OF PERCEPTION[1]

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This paper outlines a stage in the development of a general theory of perception. Underlying this development are certain notions concerning the place of theory in psychology. There is a growing conviction among psychologists that theory does not function simply as a rigid frame into which facts are to be fitted. On the contrary, theory construction is a continual process of dynamic interrelationship between scientific thought and empirical research.[2] Once constructs have evolved, the next step is to test them in the laboratory by setting up hypotheses. But this is by far not the whole story: A theory is sterile, and, so to speak, dies of old age, if the facts which support the theoretical considerations do not have the effect of generating new hypotheses which in themselves lead to further research and subsequent reformulation of theory.

Our attempts toward the development of a perceptual theory are admittedly at an early stage; our constructs are somewhat crude, the experimental situations rather elementary.

General Aim of Sensory-Tonic Field Theory: Main Constructs

Perceptual theory of today cannot limit itself to the interpretation of traditional psychophysical facts, it must do more; it must deal with facts which have been unearthed by clinical and social psychologists. In other words, it must develop basic constructs which tie together sensory, conative and cognitive factors.

In a different way it may be stated that a general perceptual theory must account for the projective nature of perception. By this we do not mean only the facts of clinical psychology concerned with so-called projective techniques, but the assumption is that any perception is essentially "projective," i.e., cognitive and conative organismic states (needs, motivation, thought) are an intricate part of perception.

This notion of the "projective" nature of perception poses one difficulty. This difficulty is concerned with the question of how two essentially alien elements such as emotion, motivation, etc., on the one side, and visual or tactual processes, etc., on the other side, can influence each other or even fuse. This problem we have called the paradox of interaction.

Let us suppose that it is true, as Bruner and others have tried to demonstrate, that the size of a coin is seen as bigger when it is highly valued. How can we understand that two factors, such as the visual factor of size and the personal factor of need, interact?

The logical solution of such a paradox must lie in the conception that these factors are only seemingly, but not actually, alien to one another.

For traditional psychology, which has dichotomized functions such as emotions, motor activity, and sensory functions into separate entities, this paradox is insurmountable. Modern organismic theory is not troubled by this difficulty because, at least speculatively, such a dichotomy does not exist. A bare statement, however, to the effect that we are

[1] The experimental work referred to is supported by a research grant from the National Institute of Mental Health, of the National Institutes of Health, Public Health Service.

[2] Closely related to this position are the views presented by Marquis in his presidential address to the APA (3).

dealing with a total organism, no matter how refined such a statement may be, is unsatisfactory. To be satisfactory such theory must be developed around constructs which are amenable to experimental manipulation. This we are trying to do. Before going into a discussion of some experimental work, we shall briefly state some of these constructs.

We have tentatively named our theoretical approach the sensory-tonic field theory of perception. It is an organismic theory and its essential tenet is that organismic states are part and parcel of perception. We have used the term "sensory-tonic" advisedly. This notion aims to overcome the paradox of interaction between sensory events and events such as motor activity, drives, and other characteristics of organismic states.

Just because visual objects send out stimuli which affect our visual apparatus, the conclusion is not warranted that the perceptual process is purely sensory. Numerous physiologists, Sherrington, Magnus, Stein, Metzger etc., have shown that sensory stimulation has an effect on muscular tonus (8, p. 94f.). Accordingly, we maintain that any stimulation whether it comes through extero-, proprio- or intero-ceptors is sensory-tonic in nature. Tonus, in traditional terms, refers to somato- as well as viscero-tonic tension.

The perceptual properties of an object are a function of the way in which the stimuli coming from that object will affect the existing sensory-tonic state of the organism. Instead of talking, as traditional psychology does, about a visual object stimulating the retina and other visual areas, we talk about a stimulus object always arousing sensory-tonic events, i.e., events which involve the total organism.

A consequence of this sort of conceptualization is the notion of equivalence of functions with respect to sensory-tonic states. Since any neuropsychological entity is neither sensory nor motor but a dynamic process prior to both, it may be affected in a similar way by stimulation through the receptors, as well as by direct stimulation of the muscles. Thus perception may be affected equivalently by various kinds of sensory stimulation and direct muscular changes. Some evidence for this will be given later.

It may be stated further that the sensory-tonic theory is a field theory. This means that there is a field constituted of two parts: body (psychophysical) and object (psychophysical). The perceptual properties of an object depend on the way in which stimuli from a physical object affect the organism and on the subsequent specific and active manner in which the organism reacts to it.

What will happen will depend on the relation between stimulus-object and existing sensory-tonic state of the organism. Principally, the stimuli may or may not interfere with the present organismic state. If the stimuli interfere, or are incommensurate with the organismic state, there emerges a tendency in the organism to change its state in the direction toward establishment of equilibrium between body and object.

We believe that the perceived properties of the object are a mirror of these dynamic relations between the object stimuli and ensuing body activity.

To summarize, we are using a number of concepts such as sensory-tonic state, functional equivalence, object-body relationship in a field, and tendencies toward equilibrium. We hope that these concepts will become clearer by a concrete discussion of experimental work.

FUNCTIONAL EQUIVALENCE: EQUILIBRATION: STUDIES OF VERTICALITY

As stated above, our ultimate goal is the development of a theory, general enough to account for the many facets of perception including its status as a factor in personality dynamics. This goal can be approached only in a stepwise fashion. Other investigators have started at a rather high level of complexity; we feel, however, that it is necessary to begin with experimental situations which, on the one hand, are concise, simple and manageable, and on the other, paradigmatic of the relationships on a higher level of complexity.

Many of our experiments deal with the perception of the vertical. We have chosen the vertical for investigation because it represents a simple case where a perceptual property (verticality) can be studied as dependent on organismic states, or better, on the relation between the object stimulating the organism and the organism reacting to the stimulation.

One of the assumptions of the sensory-tonic theory is that with respect to the total perceptual process there is a functional equivalence between sensory and muscular factors. If that assumption is sound, we should expect that stimulation through the sense organs, such as the ear, and direct stimulation of the muscles, by electrical means, at least under certain circumstances, should produce analogous results in perception. This we have demonstrated. In one of our situations, the *S* had the task of adjusting a rod in a darkroom so that it appeared vertical to him. The position at which a physical line is seen vertical is referred to from now on as "apparent vertical." In these experiments the physical position of the apparent vertical differed under various conditions. For instance, under electrical stimulation of the right neck muscle it was tilted more to the left (**6**). Auditory stimulation functions in an analogous way, i.e., one-sided application shifts the position of apparent vertical opposite to the side to which the auditory stimulus is applied (**6**). Furthermore, related results are obtained under conditions of body tilt (**11**) and rotation around the vertical axis of the body (**7**). When the body is tilted slightly to one side, the position of the apparent vertical, again, is tilted to the other side.[3] It will be noted that the effect is due to stimuli which do not come from the object; these stimuli are called "extraneous" stimuli.[4]

To understand more clearly these relationships we may ask the question, what is the vertical? We believe that one cannot conceive of a vertical line

[3] Some readers might erroneously interpret this statement to be in contradiction to that given in a preliminary exposition of the theory (**8**, p. 99). In these previous statements the displacement was discussed in terms of the perception of a plumb line (i.e., of a rod when set a true vertical); this displacement was inferred from the adjustment of the rod to the apparent vertical. In contradistinction, the statements made here and elsewhere (**5**, **6**, **7**, **9**, **10**, **11**) are in terms of the position of apparent vertical, i.e., the position of a rod when adjusted by the *S* to the vertical. As one can easily see, the perceived displacement of a perpendicular rod (plumb line) under given extraneous stimulation is opposite to that of the position of the apparent vertical. That is to say, if the position of apparent vertical is left of perpendicular, the perpendicular is perceived displaced to the right. It might be added that this change in the verbalization of the results is not coincidental but reflects rather a change in theoretical thinking forced by evidence. The emphasis has shifted from the question of how the perpendicular is perceived to the question of what is apparent verticality and what is its objective spatial position under various conditions.

[4] The effects of extraneous stimulation and postural tilts have been studied not only with respect to the visually perceived apparent vertical, but also with respect to kinaesthetically experienced vertical. The same effects have been observed (**5**).

as existing in a relatively isolated space independent of the body; the singular quality of verticality, as we see it, is due to a particular dynamic relation between the position of a line and the organismic state.

We have tried to understand this relationship through the development of the construct of equilibrium, a construct widely used in biological dynamics. Our general view is that the organism tends to establish a steady state or body equilibrium. If the organism is subjected to stimuli, one of two relationships may be present: (1) The stimuli may not interfere with the present organismic state. By this we mean that the object is, so to speak, in balance with the existing body equilibrium. (2) The stimuli may be disturbing to the body equilibrial state. Under these conditions there is a tendency to re-establish the balance. One of the several ways of re-establishing the balance is by changing the position of the object. To be more specific, when the body is erect, a perpendicular rod will be perceived as vertical because, as we believe, its stimuli do not disturb the existing body equilibrium. If, however, the body equilibrium should change, such a perpendicular rod will be incommensurate with the equilibrial state and therefore no longer seen as vertical. Now, the rod has to be shifted into an objectively tilted position in order to be in balance with the newly established body equilibrium, i.e., to be seen as vertical. In summary, if the body is tilted, or extraneous stimulation (auditory, etc.) is applied to one side, our assumption is that this induces a change in the body equilibrial state; because of this shift of body equilibrium, the line has to be shifted in order to appear vertical. This expectation, as mentioned before, is verified in the experiments.

In order to understand the dynamics underlying shifts of the position of apparent vertical due to extraneous stimulation, we may turn to neuropathological cases with instability of body balance. Patients with an injury to the cerebellum provide the best examples. In these patients, tilt of the body results frequently in falling to the side of the tilt. Similarly if these patients are stimulated asymmetrically by light or auditory stimuli (extraneous stimuli), there is also a tendency to fall to the side at which the stimulus is applied. The reason for this lack of balance is that the increase in muscular tonus due to tilt or stimulation is not properly counteracted with an increase in tonus to the other side. From this clinical picture we learn two things: first, that body tonus is changed asymmetrically through sensory stimulation in a similar way as it is through muscular action, secondly, that asymmetric stimulation needs counteractive forces to bring about a new steady state. Applying this information to our situations, a hypothetical dynamic schema can be constructed which is presented in Fig. 1. This schema is based on the assumption that extraneous stimulation increases the neuromuscular tonus to one side (primary force) and counteractive or balancing forces are brought forth in order to establish a new stable dynamic equilibrium. Such a new equilibrial state under tilt, or stimulation to the left, and tilt, or stimulation to the right, is represented in the figure.

The steps which led to this schema are summarized as follows:

(1) It is assumed that a line perceived as vertical is in a position undisturbing to the body equilibrium, or in other words, is in a position in tune with the body equilibrium.

(2) It is assumed that the existing body equilibrial state is changed in a specific manner under conditions of extraneous stimulation. Under extraneous stimulation counteractive forces come

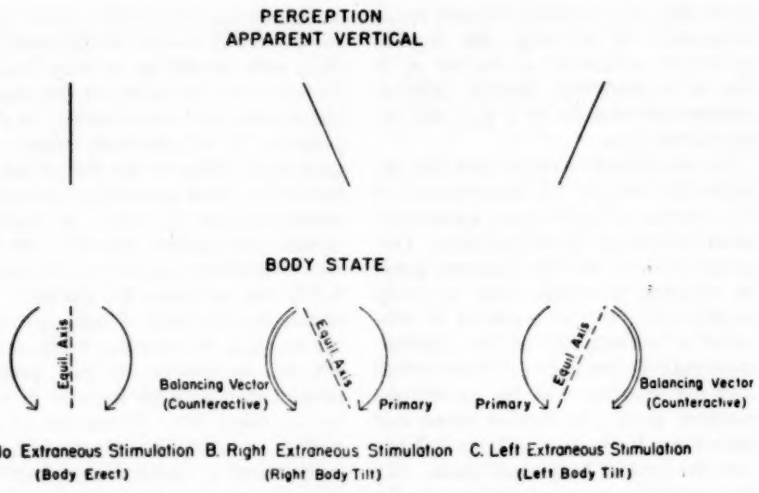

FIG. 1. Positions of apparent vertical corresponding to three body equilibrial states.

into play on the side opposite the stimulation to balance its effects. This has the consequence that a new body equilibrial state emerges. For the sake of simplicity we may characterize the equilibrial state by an axis. Thus we depict the new body equilibrial state in terms of rotation of the equilibrial axis toward the side of the counteractive or balancing force.

(3) The body equilibrial state, with its equilibrial axis is indicated by the position of the apparent vertical. The emergence of the new equilibrial state is indicated by the shift of the apparent vertical; it is a fact that the apparent vertical is tilted to the side of the counteractive force, or in terms of our representation of the body equilibrial state, rotated in the same direction as the equilibrial axis.

This diagram conveys what is meant by the statement that verticality is that position of a line which is in tune with the existing body equilibrium.

This hypothetical schema has been a valuable aid in our thinking and experimental planning. First of all, it has been productive because it has led to a number of new experimental facts. Secondly, it has helped to bring together seemingly unrelated facts such as individual equilibrial states, so-called figural adaptation, the relation between perception and motor activity, the problem of "straight ahead" or the median plane, and problems of configuration. Illustrations of these problems will now be discussed briefly.

The first of these facts to be mentioned is that of very consistent individual differences which concern the way a subject adjusts a line to the position of apparent verticality when sitting erect. For the majority of subjects the apparent vertical has a position rotated to the left; for others it has a position rotated to the right. According to our schema the consistent individual differences would mean that each individual has a characteristic steady state, or individual body equilibrium.

The next problem to be discussed has a bearing on so-called "figural adaptation" (**10**).

Up to now we have dealt principally with the results on extraneous stimulation and their interpretation. Two points have been made: (1) Shifts in body equilibrial state occur due to extraneous stimulation; (2) the shift of the position of apparent vertical is an indicator of the shift in body equilibrial state.

It should be noted that extraneous stimulation is considered to influence the body equilibrial state irrespective of the presence or absence of objects. With the introduction of an object, a new dynamic factor emerges. This factor we have identified as "object-body equilibration trend." We believe that some of the instances of "figural adaptation" discovered by Gibson and his co-workers are an expression of object-body equilibration trends (10).

Gibson and others have shown that if one views a slightly tilted line, it will appear progressively less tilted during the course of inspection, until it finally appears vertical. This so-called adaptation effect can be accounted for by the tendency of the organism toward equilibration as follows: When the body is erect, a tilted line interferes with the existing body equilibrium; subsequently, a shift in the body equilibrium is induced so that it is now more in tune with the line. We infer the nature of this shift from the results on extraneous stimulation in the following way: (1) We make the assumption that in general there is an invariant relation between position of apparent vertical and equilibrial state. (2) Given an equilibrial state induced by extraneous stimulation to the left such as that depicted in Fig. 1C a line appears perpendicular when tilted objectively to the right. (3) From the two foregoing statements we make the deduction: If a line is objectively tilted to the right but in the course of inspection becomes vertical, this happens because the equilibrial state has shifted correspondingly, namely to that depicted in Fig. 1C. Similarly, if a line tilted to the left is seen perpendicular after some time, the equilibrial state, during that time, has shifted to the equilibrial state depicted in Fig. 1B.

To repeat, we draw this inference from an assumption which is in keeping with the general principles underlying sensory-tonic theory. That is to say, there is an invariable correspondence between the position X of a line perceived as perpendicular and a particular equilibrial state Y: If we know that under induced equilibrial state Y, a line is seen as perpendicular when in position X, then, if a line in position X is seen as perpendicular, the equilibrial state Y is inferred.

The reader should keep in mind that in relating the body equilibrial states under extraneous stimulation and under "figural adaptation," we are dealing in our schema with formal relationships among balancing vectors irrespective of where they come from. In one case, the balancing vector is a function of counteraction to the effects of extraneous stimulation (body balance); in the other case, the balancing vector is a function of the disbalancing effect of object stimulation (body-object balance). Thus, in Fig. 2, which in all formal aspects is identical with Fig. 1, we have identified the nature of the balancing vectors for the case of figural adaptation.

In sum, adaptation to the vertical is here interpreted as an object-body equilibration trend.[5] Under perfect adaptation a tilted line no longer disturbs the equilibrium because the new equilibrial state is in tune with the tilted

[5] Below (p. 333) the problem of object-body equilibration trends is treated in a broader framework. There the basic assumption underlying object-body equilibration trends is identified as "the principal of minimal disturbance."

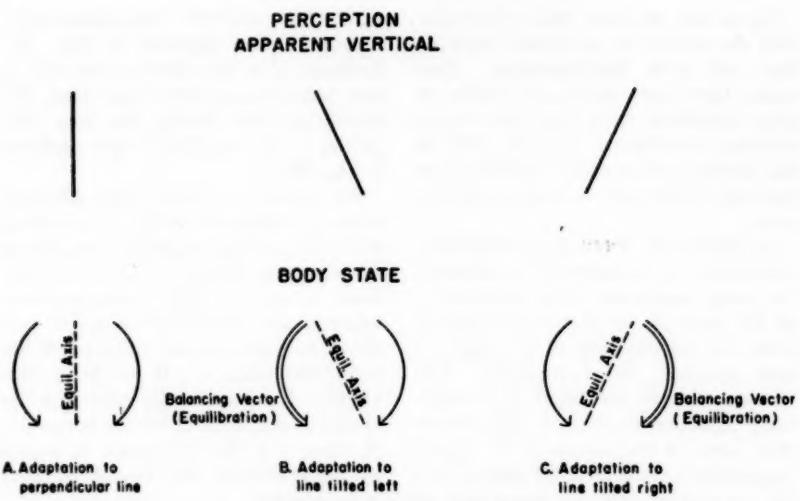

FIG. 2. Figural adaptation: Three kinds of object-body equilibration.

line. The perceptual effect is that the line now appears vertical.

Since, in most of our experiments on the vertical, we have introduced tilted objects (lines) we have to expect figural adaptation to occur. In some of these experiments, for instance, the subject has the task of adjusting to the vertical a rod which is initially tilted. In half of the trials the rod is initially tilted to the left; in the other half of the trials it is tilted to the right. We find that the subject adapts himself to the initial tilt, with the consequence that the apparent vertical is always closer to the tilted position from which the rod was started.

INTERACTION STUDIES

The principal results on the effects of extraneous stimulation, body tilt, etc., around which our discussion has centered may now be presented in a unified way. The direction of the influencing factors may be described by vectors. Vectors are introduced into our hypothetical schema to indicate the direction and magnitude of the shift of the equilibrial state (equilibrial axis). These vectors are depicted in conformity with the results obtained under the various test conditions. The present state of our investigation does not warrant specification of the magnitude of the vectors.[6] The influencing factors and their vectors are summarized in Table 1. There, "+" means a right vector or a tendency to shift the equilibrial axis and the position of the apparent vertical to the right of the perpendicular, and "—" means a left vector or a tendency to shift the equilibrial axis and the position of the apparent vertical to the left of the perpendicular. It can be seen from the table that there is functional equivalence of extraneous stimulation left, body tilt left, and starting position right; similarly, of ex-

[6] In general, in the discussion which follows, the vectors are treated as being of the same magnitude with the exception of the vector for the body tilt factor. We have clear evidence that this vector has a greater magnitude than those of the other factors involved in these experiments.

TABLE 1

FACTORS INFLUENCING VERTICALITY

Exper. Conditions		Vectors	
		→ +	← −
Extraneous Stimulation	Left	+	
	Right		−
Body Tilt	Left	+	
	Right		−
Starting Position (Adaptation)	Left		−
	Right	+	
Individual Equilibrium			−

traneous stimulation right, body tilt right, and starting position left. Individual equilibrium, as noted previously, has predominantly a left vector.

We have designed a series of experiments to test our theoretical formulations. They are in the form of interaction studies. The principal feature of these numerous experiments consists in the analysis of the varying effects dependent on the simultaneous operation of several or all of these factors. The notion of functional equivalence leads to the expectation that the effect of a combination of two or more functionally equivalent factors is the summative resultant of the vectors involved.

As an illustration, the results of one of these interaction studies will be presented. In this study there was simultaneous operation of the following factors: body tilt, electrical stimulation, starting position, and individual equilibrium. If our notion of functional equivalence of these factors is correct, then, disregarding their magnitude, we are to expect that in each case the perceptual result should be a summative effect of all the vectors present in a particular experimental situation. Therefore, if the various vectors involved have the

TABLE 2

INTERACTIVE EFFECTS: INDIVIDUAL EQUILIBRIUM, STARTING POSITION, EXTRANEOUS ELECTRICAL STIMULATION, BODY TILT

Combination of Vectors							Mean Adjustment of Rod	
Indiv. Equil.	Starting Position		Elect. Stimul.		Body Tilt = 15°		Men	Women
← −	Left ← −	Right → +	Left → +	Right ← −	Left → +	Right ← −	(N = 20)	(N = 20)
−	−			−		−	−7.4	−7.7
−	−		+			−	−5.1	−5.2
−		+		−		−	−5.8	−6.0
−		+	+			−	−4.4	−4.2
−	−			−	+		+3.5	+2.4
−	−		+		+		+5.0	+3.6
−		+		−	+		+3.6	+2.8
−		+	+		+		+6.1	+4.2

same direction, the equilibrial axis and thus the position of the apparent vertical should show a greater shift to that side than where the vectors are not of the same direction (Table 2). The most extreme differences are expected if all the vectors in one situation are to one side and all the vectors in the other situation are to the other side. The signs on the left side of the table indicate the vector direction: "−" to the left and "+" to the right. On the right side the empirical findings are presented; analogously the signs attached to the numbers indicate whether apparent vertical is tilted to the left (−) or to the right (+) of perpendicular. The numbers indicate the degrees of deviation of apparent vertical from objective vertical.

The table shows clearly that where all vectors are "−", the apparent vertical is tilted most to the left (− 7.4° for men; − 7.7° for women), and where the majority of vectors are "+" the apparent vertical is tilted most to the right (6.1° for men; 4.2° for women). Since we know from previous experiments where the variables have been studied singly that body tilt has an effect significantly more potent than the other variables, the table has to be examined separately for right and left body tilts. Within each body tilt the results closely follow the expectation: For right body tilt, with minus vectors for electric stimulation and starting position, the apparent vertical is tilted most to the left (− 7.4° for men; − 7.7° for women): where these factors have plus vectors, the apparent vertical is least to the left (− 4.4° for men; − 4.2° for women). Where these two factors have opposite vectorial signs, the shifts of the apparent vertical are intermediate. Analogous relationships hold for left body tilt.

In the results of the numerous interaction studies in which combinations of these and other variables have been studied in our laboratory, the predictions are verified to a surprisingly high degree. These interaction studies, of which the one described above is representative, provide the best evidence we possess at present for the adequacy of some of our basic theoretical constructs.

ANOTHER DIMENSION OF SPACE: THE MEDIAN PLANE

Further evidence of the usefulness of the constructs discussed is their applicability to another dimension of space, *viz.*, that of the median sagittal plane which may be defined as that position in space which is experienced as "straight ahead."

Experiments have been conducted which demonstrate the effect of extraneous stimulation on the apparent median plane. The task for the *S* was to indicate a point straight ahead of him in the fronto-parallel plane. This position was explained to the *S* as a straightforward projection of the point of *S*'s nose on the fronto-parallel plane. Analogous to the experimentation of verticality, the task was carried out under conditions of asymmetrically applied extraneous light stimulation. The stimulation was provided by specially built goggles with light bulbs inserted within each of the two eye pieces.[7] It was found that there is a significant difference in effect of right and left eye stimulation. Stimulation to the right shifts the apparent median plane more to the left and vice versa. As one can readily see, extraneous stimulation operates analogously for the median plane and the vertical. The dynamic mechanism hypothesized for the effects of extraneous stimulation on verticality may therefore be applied to the interpretation of these median plane effects.

[7] It may be mentioned that this effect is similar to that of a Ganzfeld possessing a brightness gradient.

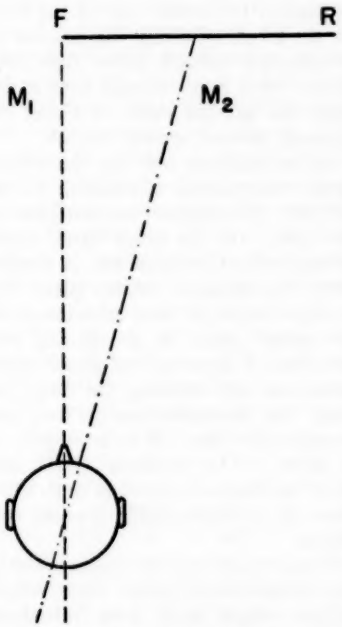

FIG. 3. Diagrammatic representation of shift in apparent median plane under asymmetrical object position.

(View from above) Asymmetrical position of rectangle, R, to the right is depicted here. Apparent median plane shifts from position M_1, toward the center of the rectangle, M_2. This has the perceptual effect that the rectangle is seen displaced to the left.

Quite another kind of effect on apparent median plane was brought out by the following experiments suggested by Roelofs' (8, p. 96) observations.[8] A homogeneously luminescent upright rectangle presented in a dark room had to be adjusted by *S* in such a way that either the left edge or the right edge was in the apparent median plane. This means that when the left edge is fixated, the figure extends entirely to the right of fixation line (asymmetrical object position right); and when the right edge is fixated, the figure extends to the left (asymmetrical object position left). There is a significant difference in apparent median plane depending on which edge is fixated. When the left edge is fixated the apparent median plane is shifted to the right, *viz.*, toward the center of the rectangle; and when the right edge is fixated the apparent median plane is shifted to the left, *viz.*, toward the center of the rectangle. An illustration is given in Fig. 3. In subsequent experiments it has been found that these effects do not depend on homogeneous illumination of the figure. When a contour rectangle rather than a solid one is used, the described effects are the same.

These effects can be interpreted as analogous to the figural adaptation effects discussed above for the vertical dimension of space. As one will remember, adaptation to the vertical was interpreted as an equilibration trend. A tilted line initiates a change in the equilibrial state of such a nature that the objectively tilted line and the body equilibrial state become in "tune" with one another. Consequently, the objectively tilted line appears vertical. Applying this reasoning to the shift of the apparent median plane, the following interpretation can be made. The observed shift of the apparent median plane toward the center of the figure is interpreted as a tendency of the organism toward "symmetrization." That is to say, "symmetrization" is an expression of the tendency of the organism to change its equilibrial state in such a way that the stimuli coming from the object will make for minimal disturbance of the organism. Thus, in our interpretation, "verticalization" (adaptation of a tilted line to the vertical) and "symmetrization" (adaptation of an

[8] The reader will note that the tentative explanation given for the results of Roelofs' experiment in a previous paper (8) has been abandoned and replaced in consequence of new facts discovered during our own experimentation.

TABLE 3

OPPOSITE EFFECTS OF TWO KINDS OF STIMULI: OBJECT STIMULI AND EXTRANEOUS (GROUND) STIMULI

Direction of Application	Kind of Stimulation	Vectors*	
		Vertical	Median Plane
LEFT	Extraneous Stimulation	+	+
	Stimuli from Object	−	−
RIGHT	Extraneous Stimulation	−	−
	Stimuli from Object	+	+

* For the vertical, a plus vector means that the stimulation results in a clockwise angular displacement of the apparent vertical in the fronto-parallel plane (around the anterior-posterior horizontal axis of the body). For the median plane, a plus vector means a clockwise angular displacement of the apparent median plane around the vertical axis of the body. Analogously, the displacements in the counterclockwise direction are indicated by minus vectors.

asymmetrical figure to an apparent symmetrical position) are considered here as two manifestations of the principle of minimal disturbance.[9]

FIGURAL AND GROUND STIMULI

If one scans the many experimental results of our studies, one is struck by a systematic difference between the effects of so-called extraneous stimulation and those of stimuli coming from objects. In direction, these effects are opposite to each other. For instance, extraneous stimulation to the left side of the body has the effect of tilting the position of the apparent vertical to the right side. On the other hand, stimuli from a line tilted left has the effect of tilting the apparent vertical toward the left.

Similar relations hold for the median plane. Extraneous stimulation to the left shifts the apparent median plane to the right. On the other hand, asymmetrical object position left of fixation shifts the apparent median plane left. In other words, in these experiments on the median plane we are dealing with two kinds of asymmetrical visual stimulation, the one affecting the body *qua* "flux," the other affecting the body *qua* "configurated flux," or as an object out in space. The organism deals with these two kinds of stimuli in ways which show up in diametrically opposed end-effects.

These effects may be summarized by the utilization of "plus" and "minus" vectors which have been introduced above. Table 3 summarizes these differences by vectors which are operationally defined in terms of the effect of the various types of stimulation.

These differences in effects of the two types of stimuli prepare the way for a new kind of behavioral analysis, that of the behavior of the organism depending on whether the adjustment occurs with reference to objects or with reference to stimuli not experienced as objects. "Objects" are defined for the subject through certain characteristics. Outstanding among these characteristics are: (1) definite location in space, and (2) contour. In contrast there are stimuli which lack contour, and definite location in space. These stimuli, previously called extraneous, can be thus considered as ground stimuli.

Object stimulation presupposes articulation between object and body. The organism copes with object stimuli in terms of equilibration tendencies affect-

[9] The terms "minimal disturbance" and "in tune with," as used in describing the dynamic happenings when an organism is faced with object-stimuli, represents a basic assumption behind our conception of what we have called the "field." This assumption and its implications for the field theory will be made more explicit in a future publication.

ing the relation between both. Ground stimuli do not have object properties. Thus, object-body equilibration tendencies are absent. Ground stimuli affect the organismic state irrespective of the presence or absence of objects. For instance, if extraneous stimulation is asymmetrically applied, counterbalancing forces come into play resulting in a new body equilibrial state. The changes in perception of objects come about because of organismic changes which as such occur whether the object is present or not. We may express this difference in another way by referring to homeostasis. The effects of ground stimuli pertain to internal homeostasis; the effects of object stimuli refer to homeostasis of the field comprised of object and body. This analysis, we hope, will eventually provide the basis for an organismic conception of the difference between figure and background which Gestalt psychologists have analyzed mainly in phenomenological terms.

VICARIOUSNESS

The notion of dynamic equivalence of sensory and muscular factors leads to a related construct, that of vicariousness of functions. By vicariousness is meant that available energy, i.e., sensory-tonic energy, may be released through different channels. It is postulated that the available sensory-tonic energy may, for instance, be released either through muscular-tonic activity of movement, or through perceptual activity.

Several studies may be mentioned which suggest such vicarious relationships between body action and perception.

One of these investigations deals with the perception of autokinetic motion under conditions of various degrees of body activity (1). The underlying hypothesis of this experiment was that if sensory-tonic energy is blocked from being released through bodily-motor channels it should find expression in heightened perceptual motion. Contrariwise, if this energy is released through greater motor activity, the degree of perceptual motion should be reduced. Observations of a fixed pin point of light in the darkroom were made under three conditions: (1) Immobilization, (2) control, and (3) increased motor activity.

Immobilization was achieved by strapping *S* into a chair for a period of 10 minutes preceding and during the test trials. Under control conditions, the *S* sat at ease, and under conditions of increased activity, the *S* had to move his arms continually while making the observations.

The principal measure was the time taken before autokinetic motion was reported. As predicted, the time taken to perceive motion was shortest for the immobilization condition, next for the control condition, and longest for observations under heightened body activity. Further results were obtained for another criterion, duration of autokinetic motion, leading to the same conclusion.

Another experiment dealing with vicariousness was reported by Korchin, Meltzoff, and Singer at the Chicago meetings (1951) of the American Psychological Association (2). Following the same hypothesis, these investigators inhibited motor activity preceding the presentation of a Rorschach card. Under these conditions, the number of movement responses were more numerous than under conditions where motor activity was not inhibited.

Another experiment—conducted at Clark—shows a similar relationship of vicariousness. Subjects were required to report what they saw when line drawings such as a train, baseball player, etc., were presented tachistoscopically. Before the pictures were exposed, the

experimental group had to press vigorously against a push-board. After pushing against the board, the *S* stood still and looked at the exposed picture. The reports of the experimental group were compared with those of a control group which made observations without preceding muscular tension. The number of movement responses reported by the experimental group was significantly smaller than that reported by the control group.

The problem of vicariousness also has bearing on some experiments which were undertaken for the purpose of relating individual directional preferences in walking and individual differences in the perception of verticality. Preliminary investigations suggest that individuals are consistent in the direction of walking while blindfolded and also behave consistently in adjusting a luminescent rod to apparent vertical in the dark. Without going into further details, the concept of vicariousness suggested the hypothesis that there is an inverse relation between walking direction and angular displacement of the apparent vertical. In keeping with this hypothesis it has been found that for right-walkers the position of apparent vertical is angularly displaced more to the left than for left-walkers (9).[10]

One will easily recognize the relation between the concept of vicariousness and that of energy transformation as it appears in psychoanalysis. We are in the process of studying differences in the way such channelizing of energy occurs typically in individuals. The possibility remains, then, that a concept such as energy transformation may become amenable to rigid experimental test.

[10] These very tentative results need further verification. One of the difficulties we are now attempting to overcome is that of obtaining a large enough field which is level and relatively noiseless.

CONFIGURATION AND PERCEPTUAL DYNAMICS

Up to now we have dealt with the relation between objects and organismic states by utilizing simple spatial properties of objects such as verticality. It is one of our goals to deal with more complex properties of perceptual objects, in traditional terms called configurational characteristics, such as the difference between a triangle and a square, and a triangle and a circle etc. or more complex figures.

As one knows, Gestalt psychology has made tremendous contributions in this area. However, for the most part, Gestalt psychology handled the problem of configuration in terms of structure rather than dynamics. The problems of Gestalt psychology were mainly those of part and whole, reorganization, and formal laws of organization such as autochthonous laws. Wherever the problem of perceptual dynamics was treated, it was either in terms of phenomenological analysis or in terms of electrophysiological or electro-chemical models. We are beginning to attack the problem of configuration in terms of object-organism relations.

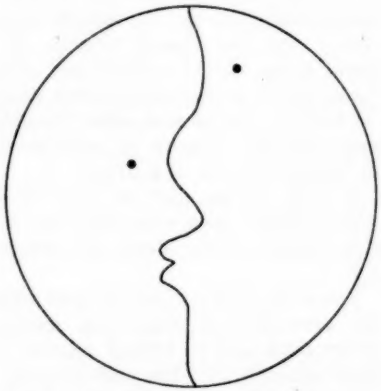

FIG. 4. Ambiguous form. Adapted from Schafer and Murphy (4).

In order to clarify the problem we may start with a discussion of an ambiguous figure (Fig. 4). This figure may be seen as a face looking toward the left, or as a face looking toward the right. Here we are dealing with changes in formal organization, but moreover, with differences in "visual dynamics." In terms of pure description it may be stated that in the one case there is a dynamic quality to the left, and in the other, to the right. While it is one thing to state one's introspections, it is quite another thing to demonstrate dynamic differences of this sort experimentally.

This problem of visual dynamics can be attacked experimentally by utilizing figural displacements in the median plane which have been described above.

Working in a darkroom, a luminescent rectangle is placed so that its right edge is in the objective median plane, i.e., so that this edge is objectively directly in front of *S*. On opening his eyes the *S* sees the edge not in the median plane but to the right of it (see previous section on median plane). If we now ask him to put this edge in such a position that it appears to be directly in front of him, he moves it to the left of the objective median plane. We describe this result by saying that his apparent median plane, or his medial equilibrial axis, has shifted to the left.

If we now substitute a right profile of a face with the nose in the objective median plane and ask the *S* to bring the nose of the face into *S*'s median plane, we find again that the apparent median plane, or medial equilibrial axis, has shifted. A similar change takes place with an arrowhead-like triangle where the apex is to the right. The crucial result is that the two triangular forms (face and triangle) are seen more to the right and, therefore, have to be shifted significantly more to the left than the rectangle (see Fig. 5).

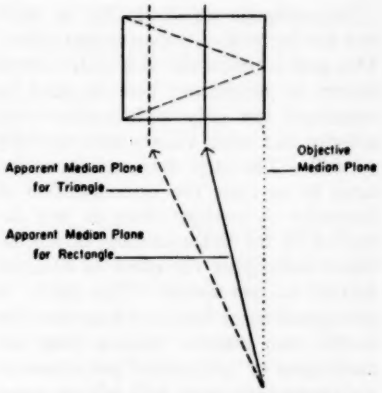

FIG. 5. Diagrammatic shift of apparent median plane to the left for rectangular and triangular form (face). It will be noted that the figures are seen as correspondingly displaced to the right, the triangle more than the rectangle.

This experimental finding is important because it demonstrates, and also gives us some understanding of, what has been referred to as "visual dynamics." We mentioned before that when people look at a right profile of a face, they experience a vectorial dynamic quality, or a directional force, so to speak, to one side. In this experiment, we believe we have demonstrated that visual dynamics is behaviorally measurable. We think we have also shown that the nature of this dynamics consists in organismic effects, i.e., in particular, in effects on equilibration tendencies of the organism.

Here we are at the threshold of understanding those dynamic characteristics of perception which have been variously called "demand qualities," "physiognomic qualities," etc. At the moment it cannot be stated what the object-body relations are which give a chair the qualities of "sitting tone" or "invitingness," but we feel that the nature of these qualities are principally not different from the dynamic qualities just described for simple configurations.

In conclusion we should like to point to a further goal of our experimentation. This goal is concerned with motivational factors in perception; here we shall be concerned not only with somato-tonic activity but with viscero-tonic activity as well. The step does not seem too large to us from the demonstration of dynamics of configurations as just described to the understanding of mechanisms underlying the effect of conative factors on perception. The shifts in perceptual space discussed here have absorbed our interest because they are paradigms of "projective" perception on an elementary level and tell us something about the object-organism relationships that, in our way of thinking, underlie perception in general.

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THE INVESTIGATION OF REPRESSION AS AN INSTANCE OF EXPERIMENTAL IDIODYNAMICS

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Experimental Idiodynamics

During the past quarter century there has been slowly struggling into existence a new experimental psychology which, in its fullest expression, is individual-centered. Such experimentation abandons the stimulus-response standpoint of behaviorism and recognizes that the stimulus is defined in the response of the subject, and that, conversely, the response is itself a stimulus which elicits further responses, including the selection of stimuli. This general approach is a result of many influences including, among others, European phenomenology, Gestalt psychology, psychoanalysis, and the projective methods. Since it is just such "individual" experimentation which must bear a chief part of the responsibility for turning the present boom of clinical psychology into something fundamental and lasting—defining clinical psychology as systematic rather than as applied—it is vital that confusion among those who engage in this type of research should not weaken the approach. It is primarily for this reason that the recent paper by Alper (1) in response to Sears and Cameron deserves reply.

There are at present in psychology at least two fundamentally different systematic orientations—that which rallies around *behavior* and that which centers in *personality*. Behaviorism, in the broadest sense of the term, with its emphasis upon the discovery of relationships between stimulus and response, exists in various versions, including those of Hull, Tolman and Skinner. But the distinction between behaviorism and psychodynamic personality theory turns not on problems of consciousness or of physiology or on the matter of other intervening variables; the basic issue concerns the fate of the individual in psychological systematization. Insofar as segments of the organism defined in S–R terms occupy the center of psychological attention, systematic psychology is a matter of behavior; when the individual person serves as the focus from the beginning to the end of the investigation, *personality*, as distinguished from *behavior*, becomes the rallying ground. In this latter approach the environmental stimulus has its place, but the central position of the individual in selecting his stimuli and in organizing his universe of responses is paramount. The general principles of this personality-oriented psychology refer largely to intra-individual dynamics and, in turn, make intelligible the relationships of the person to that environment which, in *some* measure, he shares with other persons.

In a recent formulation of this position, with the projective methods as an illustration, the writer (6) has suggested the name *idiodynamics* for the orientation which adopts the dynamics of the individual as the fundamental ground of systematization in psychology. This point of view introduces a new concept —the *idioverse*—to permit a systematic and quantitative analysis of the events in the universe of the individual. These events constitute the population to which the psychologist must attend in both his clinical and experimental work. From several sources this statement of the case for the critical importance of the individ-

ual in psychology has been begging for recognition.

One starting point for idiodynamics is the recognition of three fundamentally diverse categories of norms—universal, group and individual—which have explicitly or implicitly served to "explain" psychological data with completely different meanings for the status of the individual. It is not proposed that one of these types of norms should be exclusively accepted and the others summarily rejected; instead the ultimate reconciliation of these norms in a larger framework than we have yet had in psychology is regarded as a possible desideratum. In the light of this objective the present polemical opposition between the psychology of behavior and the psychology of personality may be regarded as transitional.

To achieve this goal will not be easy since idiodynamics operates with a vastly greater number of variables than traditional behavior theory. Moreover, there are as yet few generally accepted dynamic variables oriented toward the individual. It is all the more urgent, therefore, that "clinical" psychologists occupy themselves, as far as is practically feasible, less with applied activities and more with research which has fundamental, systematic implications centering around the individual. Research alone is not, however, the answer since much of so-called clinical research, e.g., that which is concerned with establishing Rorschach signs for some dubiously existent disease entity, is still *applied* rather than *systematic,* and is obviously test-oriented, not individual-oriented. Those personality psychologists who occupy themselves with more clearly systematic research, e.g., phenomenologists, perceptual dynamicists and experimental psychoanalysts, need to give more attention to the definition of concepts, to the exact design of experiments and of clinical investigations, and to the numerous statistical abuses which, while seeming to lend rigor to research data in idiodynamics, often lend *rigor mortis* instead. Adequate statistical innovation is here urgently needed.

In this general direction lies the reconciliation of the current unstable and unwholesome separation of "experimental" and "clinical" psychology. It may be anticipated that clinical psychology will gradually shift its focus from the applied to the systematic, and experimental psychology, already systematic, will shift its focus from segmental behavior to personality functions. The integration of clinical and experimental psychology will be found through a systematization of individual experience that may perhaps fittingly be called *experiential psychology.*

An Example: "Time-Success" in Experimental Repression

The interdependence of imposed experimental conditions and super-imposed personality variables, which the idiodynamic approach postulates, is aptly illustrated in an aspect of the experimental results reported by Rosenzweig and Mason (8) in their analysis of possible artifacts. Though limited in scope, the example may be instructive. In considering the extent to which time spent on the puzzles by the individual subjects may have affected the differences in recall—time having been allowed to vary with the subject and with the task but with measurements being made—it was shown incidentally that duration varied with the preferred mode of recall. The possible artifact thus actually proved to provide unexpected support of the main experimental results. This analysis of time in recall, originally reported in fine print, though extensively with two tables (8, pp. 261-64), has passed unnoticed

THE TIME-SUCCESS DIMENSION IN SUBJECTS WITH DIFFERENTIATED RECALL PREFERENCES

	% Long Failures Recalled	% Short Failures Recalled	% Short Successes Recalled	% Long Successes Recalled
Recall Successes > Failures	31	70	76	80
Neutral Recall	56	71	69	38
Recall Failures > Successes	82	74	62	31

in the various subsequently reported investigations of repression.

The table summarizes the experimental results for the three groups of subjects—those who recalled more successes than failures, those who showed no preference in recall, and those who recalled more failures than successes. For each subject in each of these groups the median time spent by him in working the puzzles was determined and on this basis "long" and "short" (above or below the median) were defined *for him*. The next step was to combine the time-length as experienced by the subject with the "objective" outcome of the tasks, i.e., success or failure. In consequence it was possible to calculate four figures for each of the three subject groups—the percentages of long failures, short failures, short successes, and long successes recalled.[1]

[1] The original data of this experiment are, unfortunately, no longer available; otherwise, in accordance with the recommendations below concerning the critical importance of intra-individual measures, the data would be further analyzed in order to reveal the frequencies with which *individual subjects* in the recall groups exemplified the patterns shown in the table. The present table is merely a revision (and clarification) of Table IV in Rosenzweig and Mason (8).

Examination of these figures reveals a regular pattern for the subjects who preferred successes in recall, an exactly opposite one for those who preferred failures, and a random order for the neutral recall group. The first group of subjects, preferring successes for recall (in keeping with the repression hypothesis), would seem to have construed long successes as least frustrating (high tension ultimately discharged), short successes as second, short failures as next, and long failures as most frustrating (high tension undischarged). The figures for recall are thus found in descending order from 80 per cent to 31 per cent. An exactly opposite pattern of results is obtained for the subjects who preferentially recalled failures. For the difference between these two recall groups the chi square test yields a figure of 46 and a P value of $<.01$.

Interpretation may tentatively be made from these incidental findings that time as such varies in its meaning for the subject in terms of other personality determinants—in this case, attitudes toward success and failure as related to memory. An idiodynamic definition of duration becomes relevant—as contrasted with the *universal norms* for time ordinarily employed in traditional learning and memory theory, and with the *group norms* which prevail when time is used in the clinical determination of "memory quotients." To a modest extent, albeit incidentally and in need of verification, the significance of individual norms and their reconciliation with the experimental approach may be considered to have been demonstrated.

It is of interest in this context that an investigation of duration as related to need-tension was published by Rosenzweig and Koht (7) in 1933 as part of the then current experimental repression program. This duration experiment constituted one of the earliest efforts in the

area of what has since come to be called "perceptual dynamics."

Alper's Critique

But it is time, with this background, to turn to the reply which was the "stimulus" for this paper. Let it be added that there have been other similar stimuli in the past which, for one reason or another, failed to get selected. To serve as an organizing framework, the paper by Alper in this JOURNAL (1) will be taken, page by page, as a springboard for this next section of the discussion.

Page 75: The concept of *protest* which Alper questions here is clearly differentiated, on page 259 of the original paper (8), from perseveration and repression as consisting in the need to master the environment (distinguished from socially determined "pride"). A further comment is in order to supplement Alper's discussion of Sanford (9) who raised a question regarding the relationship of maturity to ego-defensive repression. From a letter sent by the writer to Sanford soon after the publication of his paper the following may be quoted: "In my view, as in yours, repression is surely not an indication of maturity. It is, rather, an immature mechanism of ego defense. The only point to which my original study was directed in its bearing on repression concerned the stage at which the ego of the child might be sufficiently developed to be wounded by experiences of failure in the particular competition involved. The work I did with Miss Mason seemed to show that the very young children were not old enough to be affected by their failures and so would not repress them. Those approaching puberty, on the other hand, were already 'mature' enough to be vulnerable and hence were concerned about failure and, in some instances, resorted to repression *as an inadequate mechanism of ego defense*. The fact that these children were physically crippled probably meant that their egos would be weaker and later to mature in any sense of ego strength, and so might more frequently employ repression at the *same* age level as your normal subjects, once the ego had been wounded by experienced failure. As far as I can see, this statement of the matter is pretty much in line with your own thinking."

Page 77: The reply to Glixman (3) is well made as far as it goes: "The constellation of forces operating on S include *both* completion and incompletion." The intersession measure used by Glixman disregards an essential part of the experience the effects of which are to be measured, and Alper is right in regarding the intrasession measures of recall as essential. As will be shown, however, she fails to emphasize the final step —the use of intra-individual measures in this type of experiment.

Page 78: In her concern about personality structure as affecting recall, Alper overlooks the similar emphasis in Rosenzweig and Mason (8) and in Rosenzweig (5). In the former investigation differences in pride ratings for the children were taken explicitly into account and, further, an effort was made to correlate the performance of the subjects in two different experimental situations (repetition choice and recall) as an approach to personality structure. In the latter study, statements on the effects of differences in individual personality were explicitly included (5, pp. 67, 72). But the significance of the *environmental conditions* was also recognized as was, even more importantly, the *interdependence* of these conditions and the contribution of the individual personality. The two investigations by Rosenzweig were, in fact, related to each other on this very basis as homologues. To quote from the second paper (5, p.65): "In it [the present experiment] an attempt

was made to control the external experimental situation so as to reproduce with two groups of 30 adults each the alternative conditions which had obtained with the children by virtue of certain age-linked differences in their developing personalities." It is this essential interdependence which Alper neglects.

Page 79: The assumption that an unselected sample of *Ss* would "yield no statistically significant differences between the total number of completed and the total number of incompleted tasks recalled if, experimentally, there is an equal number of completed and incompleted tasks to be recalled" can have validity only if the experimentally imposed conditions are specified as neither ego-oriented nor task-oriented. Alper fails to include this qualification in her Hypothesis I and thus overlooks the interdependence just discussed. Though she later takes differences in stressfulness of the experimental situation into account, she appears to do so only to highlight the importance of differences in personality structure. In point of fact, neither aspect of the idioverse can be regarded as prepotent.

Page 79: The scrambled phrases to be assembled into *one or more* meaningful sentences, used by Alper as tasks, are not directly comparable to jigsaw puzzles with graded steps, from a beginning to a univocal ending, presumably coordinated with degrees of experienced tension. It may be questioned whether the two types of tasks can reasonably be expected to produce similar experimental results. (This comment agrees, in part, with the criticism by Glixman cited by Alper on page 84.)

Page 79: It is evident from the *t* values reported that Alper has not used intra-individual measures as a basis for her conclusions. She rightly criticizes Glixman for employing intersession scores and correctly substitutes intra-session scores. The further step in the same idiodynamic direction she fails to take in this part of her work. (She does, of course, use such measures in the later division of her subjects into Strong Egos and Weak Egos.) But the full answer to Glixman, with respect to the *role of the individual* in experiments on repression, is to be found just in this omitted final step. This emphasis on intra-individual measures will be noted in the paper by Rosenzweig (**5**, pp. 69, 71). The basic error in Glixman's reasoning lies in his failure to recognize the individual-in-his-idioverse as constituting the field for dynamic operations like repression.

Page 81: Alper commits a fundamental error of fact in stating that in the experiment of Rosenzweig (**5**) "The predominant recall *tendency* in the informal session . . . is in favor of *completed* tasks." The findings are exactly opposite and favor *incompleted* tasks. This error obviously militates against the reconciliation among the findings of Alper, Glixman and Rosenzweig which Alper is attempting to accomplish.

Page 81, footnote: The personal discussion with the writer mentioned by Alper, as summarized by her, gives the impression that her theory helps to clarify the experimental results of Rosenzweig and Mason (**8**). Since, however, the theoretical position of that study and of the later one by Rosenzweig (**5**) is actually more inclusive and more exact than the formulation of Alper, such an interpretation would be erroneous.

Pages 85–86, and 86 footnote: Alper's dichotomy of Strong Ego and Weak Ego fails to convey the dynamic situation involved in experiments on selective recall. By avoiding the term "repression" she tends further to confuse the relevant issues.

The dichotomy is misleading (a) because it unduly stresses the personality

aspect, and (b) it introduces a pair of opposites where a continuum is undoubtedly involved. Such a continuum may well be expressed as degrees of frustration tolerance (**5**, p. 73).

By avoiding the term "repression" she avoids the theoretical implication that repression, as the forgetting of ego-wounding experiences, is a function of what she calls the Strong Ego. But the implication remains and does not augment clarification. Moreover, she fails also to recognize modes of ego defense *other* than repression (*Cf.* **5**, pp. 72–73) which involve other kinds of selective recall. It was from such a fuller conceptualization that the Triadic Hypothesis (**4**) emerged.

Rosenzweig, Alper and Glixman; Sears

It should be clear from the preceding discussion that individuals differ in their sensitivity to threat, i.e., in frustration tolerance. They differ also in their modes of ego defense in the face of effective threat. But the experimentally imposed conditions for producing threat or stress are equally relevant and, as above emphasized, it is the interdependence of the experimentally imposed conditions and the contribution of the individual personality which must, in the last analysis, serve as the basis for interpreting experimental results.

In this light it is difficult to see why anyone should expect similar results, let alone identical ones, from three experimental situations as widely different as those of Alper (**2**), Glixman (**3**) and Rosenzweig (**5**)—even if one passes over the crucially important differences in the personalities of the individual subjects. In the work of Rosenzweig the subjects were individually examined and, in the stress situation, only the experimenter was present in the room with the subject; jigsaw puzzles with a univocal solution were employed as the tasks. Alper also examined her subjects individually but she included two accomplices in the experimental stress situation to provide social competition; she used scrambled sentences with *multiple* solutions as the tasks to be performed. Glixman examined his subjects in a classroom atmosphere by a group administration method and employed paper-and-pencil problems in leaflets as tasks. (This list of differentia could readily be multiplied and the confusion further compounded if the experiments of the various other investigators of repression were to be considered.)

The moral lies in two directions—that investigators in this field should aim at something more closely approaching standardized conditions, and that, at least, they have due regard for gross differences in experimental conditions before attempting to generalize from their work to that of others.

By the same token it behooves reviewers, who speak with some authority, to exercise fitting circumspection before making drastic recommendations. Sears (**10**, p. 113) writes as follows: "Neither the logical implications nor the empirical effects of interruption are sufficiently known to permit any effective control of this extraneous but intrusive factor. Its only advantage is that it provides a means of making subjects fail. Since there are other methods of doing this, methods that do not activate any process already known to influence the dependent variable under study, there appears to be little reason for the continued use of the interruption technique. When a research operation requires as much discussion of its 'psychological meaning' as interruption does, it is time to find a new operation." This appraisal with its recommendation overlooks the essential consideration that repression is by its very nature a matter of interrupted activity. The essence of the alleged phe-

nomenon lies in a *failure of expression or of integration* through the need for ego defense. That which is interrupted is consciously forgotten but is, according to repression theory, unconsciously remembered: it persists despite the evidence of conscious report and seeks closure in devious, substitute ways (*Cf.* **5**, pp. 71–2). To affirm, therefore, that the "only advantage [of the interruption technique] is that it provides a means of making subjects fail" is psychologically obtuse. Far from being "extraneous," the effects of interruption are intrinsic to the repression process.

Conclusion

Experimentation on repression and related concepts must invariably consider the idioverse of the subject—the balance of experimental conditions and personality variables as these are blended in the dynamic experience of the person. The "projective" variability of the "stimulus," for example, must be recognized from the start, permitted but controlled, and anticipated for the eventual analysis of the results. Research of this kind will perhaps always be approximate and partial, and will have to be supplemented by genetic studies of the individual as observed in favorable situations. That it is possible, however, to carry out experiments in an idiodynamic framework can hardly be doubted, given sufficient ingenuity in devising methods of investigation and of data analysis.

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